

Contract No.: 500-94-0062  
MPR Reference No.: 8246-080

94-082  
94-082-03-TJN118.11

**Preliminary Report:  
The Impact of Prospective  
Payment on the Cost  
per Episode: Striking  
the Balance Between  
Decreasing Use and  
Increasing Cost**

*July 22, 1999*

*Valerie Cheh  
Christopher Trenholm*

*Programmers:*

*Elizabeth Stuart  
Amy Zambrowski*

Submitted to:

Office and Research Demonstration  
Health Care Financing Administration  
7500 Security Blvd., (C-3-21-06)  
Baltimore, MD 21244-1850

Project Officer:  
Ann Meadow

Submitted by:

Mathematica Policy Research, Inc.  
P.O. Box 2393  
Princeton, NJ 08543-2393  
(609) 799-3535

Project Director:  
Valerie Cheh

## CONTENTS

Chapter	Page
	EXECUTIVE SUMMARY ..... ix
I	THE PER-EPISODE HOME HEALTH DEMONSTRATION AND EVALUATION ..... 1
	A. THE MEDICARE HOME HEALTH BENEFIT ..... 2
	B. THE PER-EPISODE DEMONSTRATION ..... 5
	1. Demonstration Payment and Incentives ..... 6
	2. Other Demonstration Procedures ..... 11
II	DATA AND METHODOLOGY ..... 15
	A. DATA ..... 15
	1. Constructing Episodes ..... 15
	2. Aggregating Episode Data to Agency-Level Analysis ..... 31
	3. Cost Per Visit ..... 32
	4. Outcome and Control Variables for Agency-Level Analysis ..... 35
	B. ANALYTICAL METHODS ..... 37
	1. Agency-Level Analysis: DD Models ..... 40
	2. Episode-Level Analysis: Levels Regression Models ..... 43
	3. Subgroup Analysis ..... 45
	4. Hypothesis Tests for the Impact Estimates ..... 48
	5. Robustness ..... 49
III	CHARACTERISTICS AND TRENDS OF THE AGENCIES PARTICIPATING IN THE DEMONSTRATION ..... 51
	A. WHAT TYPES OF AGENCIES ENROLLED IN THE DEMONSTRATION? ..... 51
	B. WHAT WERE THE TRENDS PRIOR TO ENROLLMENT IN THE DEMONSTRATION? ..... 53

## CONTENTS (continued)

Chapter	Page
IV	IMPACTS ON COST PER EPISODE IN THE FIRST YEAR . . . . . 59
	A. DID PROSPECTIVE PAYMENT AFFECT THE COST PER EPISODE? . . . . . 60
	B. HAVE CHANGES IN PATIENT CHARACTERISTICS AFFECTED COST PER EPISODE? . . . . . 64
	C. HOW DID AGENCIES ACHIEVE THE REDUCTION IN COST PER EPISODE? . . . . . 67
	1. Effects on Cost Per Visit . . . . . 68
	2. Effects on the Number of Visits Per Episode . . . . . 77
	3. What Was the Relative Impact of These Two Effects? . . . . . 81
	D. DID DIFFERENT TYPES OF AGENCIES RESPOND TO THE DEMONSTRATION DIFFERENTLY? . . . . . 84
V	SUMMARY AND CONCLUSIONS . . . . . 91
	A. KEY FINDINGS . . . . . 91
	B. POLICY IMPLICATIONS . . . . . 92
	C. LIMITATIONS OF THE ANALYSIS . . . . . 94
	REFERENCES . . . . . 97
	APPENDIX A: SUMMARY OF CASE-MIX ADJUSTMENT FOR PAYMENTS DURING THE DEMONSTRATION . . . . . 99

## TABLES

Table	Page
II.1 CONTROL VARIABLES FOR EPISODE-LEVEL MULTIVARIATE ANALYSIS, BY SOURCE .....	19
II.2 WEIGHTED MEANS FOR EXPLANATORY VARIABLES BY TREATMENT STATUS, AND TESTS FOR DIFFERENCES IN TREATMENT AND CONTROL GROUP MEANS .....	27
II.3 NUMBER OF AGENCIES PROVIDING DATA, BY DEMONSTRATION YEAR .....	33
II.4 MEAN VALUES OF EXPLANATORY VARIABLES USED IN AGENCY-LEVEL ANALYSIS .....	38
III.1 AGENCY CHARACTERISTICS AT THE BEGINNING OF THE DEMONSTRATION .....	52
III.2 TRENDS IN THE VOLUME OF HOME HEALTH VISITS PER AGENCY .....	54
III.3 TRENDS IN AVERAGE COST PER VISIT .....	57
IV.1 IMPACTS ON COST PER EPISODE .....	62
IV.2 COMPARISON OF IMPACTS ON COST PER EPISODE UNDER ALTERNATIVE SPECIFICATIONS .....	65
IV.3 IMPACTS OF THE DEMONSTRATION ON AVERAGE COST PER VISIT .....	69
IV.4 IMPACTS OF THE DEMONSTRATION ON COST PER VISIT, CONTROLLING FOR VOLUME AND PATIENT CHARACTERISTICS .....	71
IV.5 IMPACTS OF THE DEMONSTRATION ON AVERAGE COST PER VISIT, CONTROLLING FOR AVERAGE VISITS PER EPISODE .....	74
IV.6 IMPACTS OF THE DEMONSTRATION ON AVERAGE COST PER VISIT, EXCLUDING OUTLIERS .....	76

**TABLES** *(continued)*

<b>Table</b>	<b>Page</b>
IV.7 IMPACTS OF THE DEMONSTRATION ON THE NUMBER OF VISITS PER EPISODE .....	78
IV.8 IMPACTS OF THE DEMONSTRATION ON THE NUMBER OF VISITS PER EPISODE (EPISODE-LEVEL ANALYSIS) .....	80
IV.9 IMPACT OF PER-EPISODE PAYMENT ON THE COST PER EPISODE, BY WHETHER THE AGENCY IS FOR-PROFIT OR NONPROFIT .....	86
IV.10 IMPACT OF PER-EPISODE PAYMENT ON THE COST PER EPISODE, BY WHETHER THE AGENCY HAD A HIGH-USE OR LOW-USE PRIOR PRACTICE PATTERN .....	87
IV.11 IMPACT OF PER-EPISODE PAYMENT ON THE COST PER EPISODE, BY WHETHER THE AGENCY IS SMALL OR LARGE SIZE .....	88
IV.12 IMPACT OF PER-EPISODE PAYMENT ON THE COST PER EPISODE, BY WHETHER THE AGENCY IS HOSPITAL-BASED OR FREESTANDING .....	89

## FIGURES

Figure	Page
III.1	TRENDS IN STAFFING LEVELS ..... 55
IV.1	DISTRIBUTION OF IMPACTS ON COST PER EPISODE ..... 63
IV.2	DECOMPOSITION OF IMPACTS ON COST-PER-EPISODE ..... 83

## EXECUTIVE SUMMARY

Congress mandated in the Balanced Budget Act of 1997 that the Health Care Financing Administration (HCFA) implement a prospective payment system for home health care in 1999. HCFA has recently implemented the Per-Episode Home Health Prospective Payment Demonstration, which tests one potential prospective payment method. Under the demonstration, participating home health agencies are paid a fixed lump sum for the first 120 days of each episode of care provided to Medicare beneficiaries and a predetermined rate for each visit thereafter. This method of compensation differs substantially from the retrospective cost-based method of Medicare reimbursement for home health services, under which agencies are reimbursed for actual costs incurred, up to a specific limit. By allowing agencies to retain most of any surplus payments over cost, prospective payment gives agencies a financial incentive to provide home health care more cost-efficiently than they do under traditional cost-based reimbursement.

Ninety-one agencies in five states entered the three-year demonstration at the start of their 1996 fiscal year. Prior to the start of the demonstration, the participating agencies were randomly assigned to either the treatment group (which is paid under the demonstration's prospective payment method) or a control group (which continues to be paid under Medicare's normal method of cost-based reimbursement). The payments that treatment group agencies receive for the first 120 days of a patient episode are based on each agency's own costs in the fiscal year immediately preceding its entry into the demonstration, adjusted for changes in its case mix. While each agency is "at risk" during the first 120 days after admission for all home health visits the patient needs, HCFA reimburses treatment agencies for up to 99 percent of fiscal-year losses. Profits in excess of specified limits must be shared with HCFA.

## RESEARCH QUESTIONS AND METHODOLOGY

In this report, we examine the available data from the first eight months of the demonstration to test hypotheses about the possible effects of prospective payment on the cost per episode. In particular, we examine whether prospective payment affected the cost of care in the first 120 days. To understand what led to any changes, we also examined the impacts of prospective payment on (1) the cost per visit, and (2) the number of visits per episode. In addition, we tested whether these outcomes differed between subgroups of agencies defined by their for-profit status, size, and other key characteristics.

Medicare claims files provided the data on the use of services during the first 120 days of home health episodes in both the base period and the demonstration period. Medicare cost reports provided the cost-per-visit data for the two periods. Data collected at admission for case-mix adjustment and from prior Medicare claims provided measures of preadmission characteristics of patients admitted to agencies during the demonstration. Data on agency characteristics were obtained from the agency cost reports and the demonstration implementation contractor.

Because the focus of our analysis is on the agency-level response to prospective payment, we generally make the agency the unit of observation and employ differences-in-differences models to compute demonstration impacts. We also conduct some analyses in which the episode of care is the unit of observation and use standard ordinary least squares regression models and demonstration period data to examine impacts. In these analyses, observations are weighted so that each agency is represented equally in the analysis.

## FINDINGS

### *Cost per Episode Fell 13 Percent*

We find that prospective payment reduced the cost per episode by \$419, or 13 percent. Among prospectively paid (treatment) agencies, cost per episode declined \$280, or about 9 percent, between the predemonstration and demonstration periods. In contrast, the cost per episode rose \$139, or about four percent, among cost-reimbursed (control) agencies. Taking the difference between these two changes yields the overall impact of \$419. This reduction does not appear to be the result of changes in the types of patients served, as controlling for patient characteristics does not affect the size of the impact.

### *Costs per Visit Increased as Visits per Episode Decreased*

The reduction in the cost per episode is the result of two countervailing forces: a 21 percent overall decrease in the visits per episode, partially offset by an 8 percent overall increase in the cost per visit. Treatment agencies significantly reduced the average number of visits provided per episode, providing 8.5 fewer visits, on average, than control agencies.<sup>1</sup> Most of this reduction was the result of fewer visits by skilled nurses and home health aides. However, prospective payment also led to estimated declines in the other services, though these were not statistically significant. Treatment agencies had significantly higher cost per visit for the two highest frequency services: skilled nursing care and home health aide care. These increased costs were largely the result of the treatment agencies' efforts to decrease service use--agencies with the greatest cuts in visits per episode had the largest increase in per-visit costs. One explanation for this is that the methods the agencies are using to reduce visits--for example, increased supervision--directly add to per-visit costs.

Understanding the relative magnitude of these impacts can be helpful for policymakers as they design the new payment system. As shown in Figure 1, the large decline in the cost per episode relative to the base period for treatment agencies (-\$280, or about nine percent) was the result of two large but offsetting effects: (1) rising cost per visit that drove the cost per episode up \$376, and (2) decreases in visits per episode that reduced the cost per episode by \$656. This suggests that, for every dollar saved from reduced visits, about 57 cents was offset by higher per-visit costs. Among control agencies, the change in cost per episode from the base period was far less dramatic. A

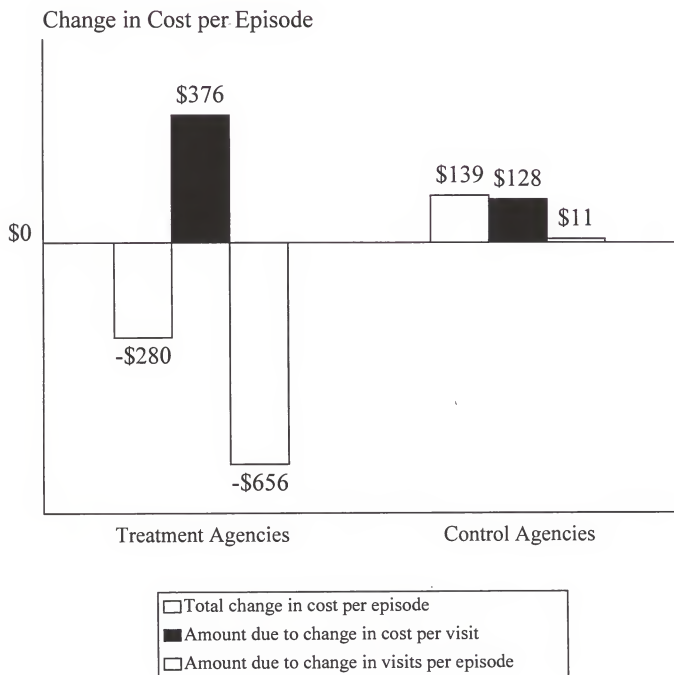
---

<sup>1</sup>This estimate differs slightly from that reported earlier mainly due to differences in sample size. A few agencies did not submit cost data in time to be included in this report.



FIGURE 1

# DISTRIBUTION OF IMPACTS ON COST PER EPISODE



Note: The amount due to the interaction of cost per visit and visits per episode is allocated proportionately between the two effects.

relatively small increase in cost per episode (\$139, or about four percent) was due to rising costs among both factors, with most of the increase (\$128) coming from higher cost per visit. Because treatment agencies' costs went down \$280 per episode instead of up by \$139, the overall effect of prospective payment is \$419.

### ***Small Agencies Failed to Cut Episode Costs***

The impact on the cost per episode is fairly similar across different types of agencies, including proprietary/nonprofit and hospital-based/freestanding. However, the impact on small agencies--those that rendered fewer than 30,000 visits in the base year--was significantly smaller than it was on large agencies. Small agencies failed to decrease their cost per episode in the first demonstration year, evidently because they increased their cost per visit more and reduced their number of visits less than larger treatment agencies.

## **CONCLUSIONS**

### ***Prospective Payment May Yield Substantial Cost Savings, but the System Must Be Flexible***

Our findings suggest that prospective payment for home health care could lead to sizable savings in costs to HCFA, depending on how the payment rates are set. These savings from the reduction in cost per episode during the at-risk period might be mitigated by changes in the patterns of care in the outlier period.<sup>2</sup> Nonetheless, according to the evidence presented here, the potential exists both for HCFA to save and for agencies to prosper.

Policymakers must remember that these agencies achieved cost savings by trading off decreases in visits for increases in per-visit costs. The old adage applies: one must spend money to make money. Supervising utilization more carefully, buying and implementing care-mapping protocols, and making telephone calls in lieu of visits all cost money. If the prospective payment system is going to work, it must be flexible enough that agencies can pay for the changes needed. The Interim Payment System (currently in effect) is an example of a system that is unlikely to achieve maximum cost savings, because it restricts flexibility. By imposing stricter limits on agencies' cost per visit, IPS effectively penalizes agencies that could achieve large cost per episode reductions using strategies that coincidentally raise per-visit costs--the exact strategies adopted by treatment agencies under the demonstration.

### ***Sparsely Populated Areas May Warrant Further Consideration***

Furthermore, the results presented here suggest that because of a combination of two problems, small agencies may have difficulty thriving under prospective payment. First, small agencies have fewer episodes, so average levels of use will be much more volatile from year to year. Although

---

<sup>2</sup>The issue of outlier payments will be addressed in future reports when data are available.

small agencies may be able to manage levels of use over the long run, in any given period, they will have greater difficulty. Second, small agencies face more pronounced economies of scale; that is, when small agencies decrease their overall volume of services, their costs per visit increase more than those of large agencies. Indeed, research has shown that economies of scale may exist only for small agencies; changes in volume do not affect the large agencies' cost per visit (Chu, Brown, and Phillips 1993).

Nevertheless, under prospective payment, some small agencies may be able to reduce their cost per episode and still remain successful. This, however, probably is not true for many small agencies because of the fixed costs associated with operating a home health agency. Some small agencies will find it in their best interest to merge with other agencies once prospective payment has been adopted. But, agencies that serve *sparsely populated* areas may have only limited opportunities to reduce the cost per episode because of their small potential client base. An efficiently run agency in a large geographic area that requires less than a minimal number of visits per year may not be able to reduce its costs to the level of larger agencies even if, on average, the agency has similar patterns of care. Thus, the risk is that small agencies may leave the business under prospective payment and limit Medicare beneficiaries' access to care.

HCFA may wish to consider a special arrangement for agencies located in areas in which they are the only Medicare home health provider and where there is an insufficient population within a specified geographic limit (for example, 50-mile radius) to require a minimal number of visits. One approach would be to implement in such areas a modified version of the profit and loss sharing arrangement used in the demonstration. This provision would motivate selected agencies to provide care efficiently while limiting the yearly swings in profitability that can affect solvency. Note, however, that any special arrangement would be difficult to implement. Such an arrangement would, for instance, require statutory authority and would create payment inconsistencies objectionable to some agencies. Moreover, any arrangement would need to be carefully designed to avoid subsidizing inefficient providers.

### ***These Results Are Promising, but Preliminary***

While the results are promising, they must be viewed as preliminary, for several reasons. First, the data used are from only the first eight months of the demonstration. Impacts may change as agencies become more adept at finding ways to provide home health care efficiently. Second, these findings provide no information on the *consequences* of the observed reduction in the number of visits per episode. Although preliminary reports have shown that the demonstration had no discernable effect on quality (Chen et al. 1998), these early analyses were based on early experience and limited outcome measures. Other issues, such as the impacts on patient satisfaction, will be addressed in future reports. Third, the potential cost savings will be affected by demonstration effects on service use in the period after the first 120 days of an episode. Effects on potential costs to HCFA also depend on whether beneficiaries' use of other Medicare-covered services is affected, although preliminary results suggest that this is not an issue (Schore 1997).

Generalizing from the demonstration results to the expected effects of a national program must also be done with caution, even when the final estimates are obtained. A national program would

likely differ from the demonstration program in several respects, which could lead to larger or smaller impacts than our estimates. Moreover, if the participating agencies are not representative of agencies nationally, the demonstration estimates may underestimate or overestimate the reductions in visits and costs that would occur under a national prospective payment system.

Despite the limitations of these preliminary estimates, the conclusion is clear: prospective payment seems to be a promising alternative to the present payment system. If our future analyses confirm these early estimates, and no adverse effects on the quality of care or access to care are observed, prospective payment may be a viable mechanism for Medicare to reverse the pattern of large increases in home health care expenditures.

## **I. THE PER-EPISODE HOME HEALTH DEMONSTRATION AND EVALUATION**

The Health Care Financing Administration's (HCFA's) Per-Episode Home Health Prospective Payment Demonstration tests the extent to which prospective payment for Medicare home health services increases efficiency in the provision of services. Such efficiency is meant to reduce public expenditures while preserving access to and quality of care. By allowing participating agencies to generate savings, per-episode payment encourages them to reduce both the number of visits per episode and the cost per visit. These incentives differ greatly from those found in the current system of cost-based reimbursement, which provides no reward for efficient care delivery.

This report presents the findings of an analysis of demonstration impacts for the early months of the demonstration on the cost of Medicare home health services during the period covered by the per-episode payment. These impacts are critical, because decreasing the cost of care during this period is the chief means by which agencies can increase efficiency and earn profits. Unless agencies increase their efficiency in providing care, the Medicare program will not save resources when prospective payment is implemented in 1999.

We caution, however, that these are preliminary results. The National Home Health Prospective Payment Demonstration lasts for three years, a period purposely selected to give agencies enough time to work through the anticipated start-up problems and to learn how to deliver care efficiently under the new payment system. Indeed, site visits during the first six months of the agencies' enrollment documented that some agencies had encountered a number of problems and had not yet enjoyed the opportunity to implement many changes that would improve efficiency (Phillips and Thompson 1997). Thus, the report's findings that are based on the first year of the demonstration may underestimate the eventual effects of prospective payment on demonstration agencies.

Despite this limitation, this preliminary analysis can provide important information for the policymaking process. Congress mandated that, beginning in 2000, the Medicare program will pay for home health services using a prospective payment system. HCFA needs to design that prospective payment system now; it cannot wait for the demonstration to be completed and all the data collected. This report provides important information on how cost per episode may change and what factors will contribute to that change.

#### **A. THE MEDICARE HOME HEALTH BENEFIT**

Congress established the Medicare home health care benefit in 1965, when the original Medicare program was created. Home health benefits were included to offer beneficiaries with acute conditions a less intensive and less expensive alternative to inpatient hospital care. At different times since the inception of the Medicare program, the home health benefit has been modified, partly to increase access to care.

Currently, the Medicare home health benefit covers home health services under Parts A and B; neither a deductible nor coinsurance applies.<sup>1</sup> To be eligible for home health benefits, the beneficiary must (1) have Medicare coverage; (2) be homebound; (3) be under the care of a physician; and (4) need skilled nursing, physical therapy, or speech therapy on a part-time or intermittent basis.<sup>2</sup>

---

<sup>1</sup>A small amount of ambulatory Part B home health services, which are subject to deductibles and copayments, are also provided.

<sup>2</sup>Skilled nursing services are covered as long as (1) a physician has ordered them, (2) they are required on a part-time or intermittent basis (The Balanced Budget Agreement of 1997 defines this as less than 8 hours a day and 28 hours per week, with some exceptions), (3) they require the skills of a registered nurse (or a licensed practical nurse or licensed vocational nurse under a registered nurse's supervision), and (4) they are reasonable and necessary to treat an illness or injury. Physical therapy and speech therapy are covered if a physician's assessment recommends them. Beneficiaries (continued...)

HCFA administers the Medicare home health benefit through fiscal intermediaries (FIs), each serving a defined geographic region of the country. In addition to serving as communication links between HCFA and the agencies, FIs also review claims to limit inappropriate use of services, determine reasonable costs, and administer payments to home health agencies.

Outside the prospective payment demonstration, Medicare reimburses agencies for the reasonable costs incurred to provide care. The Balanced Budget Amendment of 1997 defines reasonable cost as cost not exceeding to be 105 percent of the median per-visit cost for freestanding agencies.<sup>3</sup> Agencies incurring aggregate costs that exceed these limits are reimbursed only up to the limits (known as the Section 223 limits). The cost limits were frozen for reporting periods that began between July 1, 1994, and June 30, 1996.

Expenditures for home health care represent a material proportion of all Medicare expenditures (about nine percent in fiscal year 1994), and these expenditures have been growing rapidly in recent years (Health Care Financing Administration 1996). Spending for Medicare home health services has grown at least 20 percent a year since 1989, the year in which coverage was broadened as part of the settlement of a lawsuit brought against HCFA. After a 53 percent spike in annual growth in 1990, however, the rate of growth has declined (ProPAC 1996). Little of the growth is due to increases in cost per visit; rather, it is due to increases in the number of beneficiaries receiving Medicare home health care and in the number of visits per beneficiary.

---

<sup>2</sup>(...continued)

who need only occupational therapy are entitled to benefits only if they have established a prior need for skilled nursing care, speech therapy, or physical therapy in the current or prior certification period (see Teplitsky and Janson 1985-1992, p. VII.23, Section 204.4).

<sup>3</sup>During the period covered by the data in this report, an agency's costs were judged reasonable as long as they did not exceed 112 percent of the mean cost incurred by all agencies (for the agency's mix of visits) in the same geographic area.

Since the program's inception, the number of Medicare-certified agencies has more than quadrupled. In 1995, there were roughly 8,700 Medicare-certified home care agencies (ProPAC 1996). Administratively, home health agencies have different ownership and auspices. They can be freestanding for-profit, freestanding nonprofit, affiliated with a facility (such as a hospital or skilled nursing facility), or operated by a government entity. Most of the recent growth in the number of Medicare-certified agencies has been in the number of hospital-based and freestanding for-profit agencies (ProPAC 1996). The distribution of ownership/auspices varies considerably by region of the country. Government-operated and private nonprofit agencies dominate the Northeast. Freestanding, for-profit agencies are pervasive in the South and West and even dominate the markets in some states.

Similarly, the number of Medicare-covered visits per episode and the length of episodes vary widely across regions. For example, among beneficiaries admitted to home health in 1990 and 1991, the mean number of approved visits in an episode of home health care was 47, and the mean episode length was 94 days. However, the mean number of visits per episode varied from 28 in the Pacific region to 95 in the East South Central region, and the mean episode length varied from 60 days in the Pacific region to 180 in the East South Central region (Schor 1995). In 1994, the mean visits per beneficiary served was 66 nationally but varied from 45 in the Pacific region to 106 in the East South Central region (Health Care Financing Administration 1996).

The dramatic growth of home health as a proportion of total Medicare spending, combined with striking regional variation in its use and the explosive growth of the home health industry, prompted Congress to legislate changes to the Medicare home health benefit as part of the Balanced Budget Act of 1997. The act mandates the implementation of per-episode prospective payment for Medicare



home health by 1999. Other changes to the home health benefit under the Balanced Budget Act include:

- Lowering the per-visit cost limit from 112 percent of the mean cost to 105 percent of the median cost for freestanding agencies
- Determining the maximum payment for an agency with a new algorithm that is based on annual per-beneficiary costs or per-visit costs in a base year (whichever is lower)
- Eliminating coverage for blood drawing when it is the only home health service required
- Redefining "part-time" and "intermittent" care
- Redefining the payment basis from the location of the agency to the location of the patient
- Requiring additional billing information (identifier for admitting physician and visit length)

## **B. THE PER-EPISODE DEMONSTRATION**

As indicated, the current Medicare home health payment system reimburses agencies for allowable costs up to a limit based on 105 percent of the median national cost of freestanding agencies. Because there is no mechanism for home health agencies to realize profits beyond costs, this system provides no incentive for producing services efficiently and, in effect, subsidizes inefficient providers. Per-Episode Prospective Payment is meant to increase efficiency, using the opportunity to generate profits as the primary incentive.

Ninety-one Medicare-certified home health agencies in five states--California, Florida, Illinois, Massachusetts, and Texas--enrolled in the three-year per-episode demonstration.<sup>4</sup> Forty-seven of

---

<sup>4</sup>Reflecting the United States more generally, considerable variation existed in the use of Medicare home health across the five demonstration states. In 1994, the mean numbers of visits provided per beneficiary using home health were as follows: California, 46; Illinois, 52; Florida, 76; Massachusetts, 87; and Texas, 97 (Health Care Financing Administration 1996).

them were randomly assigned to the treatment group to receive per-episode payment. The remaining 44 were assigned to the control group to continue under cost reimbursement. The first agencies in the treatment group began implementing prospective payment in June 1995; the latest entrant began in January 1996. Each agency started as its fiscal year began. Demonstration operations were suppose to be completed in December 1998, but an extension has been implemented for the prospectively paid agencies. They will continue in the demonstration until the new prospective payment system is in place.

Mathematica Policy Research, Inc. (MPR) is the evaluation contractor responsible for assessing the impacts of the demonstration and its implementation. Several other organizations are participating in the demonstration. Abt Associates, Inc. is the implementation contractor responsible for recruiting demonstration agencies, monitoring the status of demonstration operations, and calculating certain statistics needed for agency payment. Palmetto Government Benefits Administrator (PGBA) is the FI responsible for review of claims and agency payment. The Center for Health Policy Research (CHPR) at the University of Colorado is the contractor responsible for designing and implementing a quality assurance system for the demonstration agencies.

## **1. Demonstration Payment and Incentives**

HCFA developed the Home Health Prospective Payment Demonstration to assess whether the profit motive can increase the efficiency of providing Medicare home health care and thereby reduce public expenditures, without sacrificing access to care or the quality of care. Phase I of the demonstration, which tested per-visit prospective rate setting, provided agencies an opportunity to generate profits (and avoid losses) by reducing per-visit costs.<sup>5</sup> The evaluation found that while

---

<sup>5</sup>The per-visit demonstration was implemented in the same five states; however, most of the  
(continued...)

agencies would make profits under this payment system, in order for the Medicare program to contain costs, Medicare must contain the volume of services. The current phase of the demonstration, Phase II, tests per-episode prospective payment. Under per-episode payment, agencies may earn profits by reducing the number of visits, as well as by reducing per-visit costs.<sup>6</sup>

#### **a. Payment**

Agencies selected for the treatment group receive a lump-sum payment for the first 120 days of home health care, regardless of number of visits provided or their cost.<sup>7</sup> The agencies are thus "at risk" for the costs of care incurred during this period. Those agencies that can provide care during this period for less than the fixed (per-episode) rate will generate profits, whereas those whose costs exceed the fixed rate will incur losses.

For each visit beyond 120 days (referred to as outlier visits), treatment agencies receive a fixed payment rate that varies by the type of visit. In the demonstration, a treatment agency is also paid on a per-visit basis for visits made to patients admitted before the agency began demonstration operations ("phase-in" visits) and to those admitted within 120 days of the end of demonstration

---

<sup>6</sup>(...continued)

agencies participating in the per-episode demonstration did not participate in the per-visit demonstration. (Only agencies in the per-visit control group were eligible.) For details on the per-visit demonstration results, see Brown et al. (1995).

<sup>7</sup>Strictly speaking, only for-profit agencies earn profits; nonprofit agencies generate surpluses. However, for brevity, we use the term "profits" in this report to refer to surpluses generated by nonprofit agencies, as well as to profits earned by for-profit agencies.

<sup>8</sup>Durable medical equipment, nonroutine medical supplies, and Part B ambulatory home health services continue to be reimbursed at cost throughout the demonstration. In addition, if an agency did not provide one or more of the six Medicare services during the base year but begins to do so during the demonstration, those visits are also reimbursed at cost during the demonstration, as are the costs of care for which Medicare is a secondary payer.

operations in that agency ("phase-out" visits). Agencies that can provide an outlier, phase-in, or phase-out visit for less than the fixed (per-visit) rate can also generate profits.

In the demonstration, home health episodes are defined by gaps of at least 45 days in Medicare-covered home health care. Only after the 120-day risk period and a 45-day gap in services can an agency receive a new per-episode payment for a given Medicare beneficiary.

#### **b. Rate Setting**

Prospective per-episode rates are based on an agency's costs and episode profile in the fiscal year preceding its entry into the demonstration (the base year), adjusted for inflation and changes in case mix in each evaluation year.<sup>8</sup> The episode profile is the average number of visits provided by the agency during an episode, calculated for each of the six types of visits covered by Medicare. Payments for outlier, phase-in, and phase-out visits are also based on the agency's base-year per-visit costs (adjusted for inflation).<sup>9</sup> HCFA's market basket is used to adjust both the per-visit and per-episode rates for inflation.

The case-mix adjuster classifies each patient into one of 18 groups on the basis of 12 variables that describe the patient's characteristics. From this information, an aggregate case-mix index is created for each agency. At the end of each year of the demonstration, an agency's case-mix index for that year is compared with its case-mix index in the base quarter (the last quarter of the base year). If the agency's case mix differs, its aggregate payment is retrospectively adjusted. See Appendix A for further details.

---

<sup>8</sup>For more information on payment rates under the demonstration, see Phillips et al. (1995).

<sup>9</sup>Because complete data for episode profiles and settled cost reports are not available for a given year until some months after that year is over, the initial lump-sum and per-visit rates used in the demonstration were preliminary and were revised as final base-year data became available.

### **c. Loss Sharing and Profit Sharing**

To encourage agencies to participate in the demonstration, HCFA provided a loss-sharing arrangement. HCFA reimburses treatment agencies for 99 percent of losses in the first demonstration year, and for 98 and 97 percent of losses in the second and third years of the demonstration, respectively, as long as total payments are within the Section 223 limits.

To counteract the incentive to reduce the quality of care to generate profits, as well as to prevent agencies from realizing windfall profits at public expense, HCFA shares in profits above a specified threshold profit rate. If the total of a treatment agency's per-episode and per-visit prospective payments is greater than the costs incurred in rendering the services covered by these payments, then any profit greater than five percent of total allowable costs for these services is subject to profit sharing with HCFA. HCFA's share of profits is 25 percent of profits if profits equal between 5 percent and 15 percent of total allowable costs. HCFA's share rises if profits exceed 15 percent (with the share of profits over 15 percent varying by demonstration year).

### **d. Incentives**

Treatment agencies can reduce the cost of care rendered during the 120-day period by (1) reducing the number of visits provided during the risk period, (2) changing the visit mix to make less costly visits a larger proportion of the total number, or (3) reducing per-visit costs (or some combination of these three). Reductions in the number of visits during the risk period could involve discharging patients earlier, thereby reducing the length of the episode or reducing the frequency of visits without reducing episode length. Reductions in the average number of visits could also be achieved by admitting a mix of patients needing less care, though this may also result in a lower payment. Reductions in per-visit costs could be achieved either by cutting direct costs (such as the length of a visit) or administrative costs (such as supervision). Alternatively, agencies might accept

increases in per-visit costs to reduce the number of visits during the risk period. For example, agencies might hire wound care specialists (who command higher salaries) in an effort to heal wounds more quickly and thereby reduce the number of visits, or they might use additional administrative resources to monitor the number of visits provided, thereby increasing per-visit costs. Per-visit costs might also increase if agencies perform in a single longer visit services that they previously provided in (and billed for as) two separate visits. In addition, as agencies reduce per-episode visits, they may experience a reduction in the direct-cost base over which their administrative costs must be spread, which may mean some loss in economies of scale. As a result, treatment agencies have an incentive to increase the number of outlier (and phase-in and phase-out) visits to help offset any volume reductions due to decreases in the number of visits during the risk period, as well as to increase the number of patients they serve.

Profit motive is the prime incentive offered under the demonstration. While treatment agencies may incur losses, the generous loss-sharing provisions of the demonstration limit the incentive for agencies to alter their behavior to avoid losses, particularly in the first demonstration year. Thus, the demonstration's incentives rely heavily on the "carrot" of profits and relatively little on the "stick" of losses.

Agencies' responses to the incentives offered by the demonstration will depend on the priority each agency places on maximizing profits, relative to other goals. Nonprofit agencies, in particular, may view their primary mission as meeting the needs of the communities they serve. Consequently, they may be more reluctant than for-profit agencies to reduce visits during the risk period on the grounds that doing so would reduce care to those in need. The demonstration, however, does provide nonprofit agencies with an opportunity to generate profits that could then be used to develop programs beneficial to their community or to provide services to those in the community who cannot

obtain them in other ways (such as through Medicaid, other public programs, or private purchase).<sup>10</sup> Given the demonstration's emphasis on profit motive, we expect for-profit agencies to respond more aggressively than nonprofit agencies to the incentive of per-episode prospective rate setting.

We also expect that hospital-based agencies may be less responsive than freestanding agencies to the opportunity to earn a profit under the demonstration. The former must respond to the hospital's need to discharge patients promptly and to "flow down" hospital administrative costs to the home health agency. Attention to the needs of the parent organization may also affect the behavior of other agencies that belong to a chain or other system of organizations.

## **2. Other Demonstration Procedures**

### **a. Medical Review**

For agencies in the treatment group, only limited medical review (known as "abbreviated" medical review) is performed by the demonstration FI for care delivered during the risk period. This review, which involves only the admission bill, seeks to determine whether the patient met the coverage criteria for home health care and whether at least one visit that met these criteria was delivered. As a condition of payment, the demonstration FI requires that the agency submit HCFA 485 and 486 forms (which contain information on the patient's health and eligibility status, as well as the home health plan of treatment) or clinical notes for admissions that coincide with an episode eligible for prospective payment. The medical review process is based on these materials.

---

<sup>10</sup>It is theoretically possible that nonprofit agencies would take advantage of the loss-sharing provisions to increase visits during the risk period, if they believe that Medicare has restricted the provision of needed care. Nonprofit agencies might treat the loss-sharing provisions as a source of community service funds, accessible with a small amount of funding (equal to one, two, or three percent of losses) from private sources.

All visits paid for under per-visit rate setting are subject to the usual focused medical review, under which a sample of claims is reviewed to ensure that each visit is medically reasonable and necessary. Medical review for control agencies continues under the current (nondemonstration) regulations. The only major difference is that control agencies are assigned to the demonstration FI. Since the demonstration FI's medical review procedures may differ in minor ways from those of other FIs, control agencies may be subject to policies somewhat different from those they are accustomed to.

#### **b. Billing**

Treatment agencies must submit an admission bill to the demonstration FI to initiate an episode of care. Treatment agencies are expected to submit interim bills for the rest of the risk period, although payment for visits is not predicated on their submission.<sup>11</sup> The agency must bill separately any outlier visits. When a patient is discharged, either during the risk or outlier period, agencies are to submit a discharge bill to terminate the episode. The FI will not initiate a new episode for a given patient if a prior episode has not been terminated. In addition, before initiating a new episode, the FI checks that the 120-day risk period and a 45-day gap have elapsed.

If a treatment admission claim is accepted (following abbreviated medical review), the per-episode payment is made as a lump sum.<sup>12</sup> While medical review is pending, subsequent episode

---

<sup>11</sup>The interim bills are required for reimbursement for supplies and for calculation of costs for profit and loss sharing with HCFA. Interim bills also provide information on number of visits required for the evaluation.

<sup>12</sup>If the admission claim is denied, interim claims for that episode are suspended for 65 days to await appeal. If an appeal is filed, interim claims are suspended also until a decision is made on the appeal for the admission claim. When an admission claim is denied and an appeal is not filed within 65 days, or if the denial of the admission claim is upheld on appeal, suspended interim claims are released for possible payment under per-visit rate setting.



bills are suspended. Initially, all episodes were subject to abbreviated medical review; in mid-1996, however, the proportion was reduced to 25 percent.<sup>13</sup>

Periodic interim payments (PIPs), which are intended to smooth cash flow for home health agencies, were originally discontinued for treatment agencies. However, a similar periodic payment system, called biweekly interim payments (BIPs), was later reintroduced to meet the cash flow needs of some treatment agencies, which had experienced delays in receiving per-episode payments.

Control agencies continue to submit bills as under cost reimbursement and continue to be eligible for PIP. The FI bases PIP payments on the agency's average cost for each type of visit, while other FIs base PIP payment on overall agency's average cost per visit. As a result, there may be minor differences in control agency PIP payments compared to what control agencies have experienced outside the demonstration.

### **c. Quality Assurance**

All agencies participating in the demonstration (in both the treatment and control groups) are required to collect and submit patient-specific information to the demonstration quality assurance contractor. The quality assurance procedures follow a continuous approach. Visiting staff from demonstration agencies are required to collect information (primarily on functional status and medical condition) at admission and again at discharge or 120 days after admission, whichever comes first. Similar information is also collected before admission to an inpatient facility (for a stay of 48 hours or more) and when the patient returns to home health care following such an inpatient stay. The quality assurance contractor uses this information to develop profiles describing patient outcomes at each agency. These profiles are provided to the demonstration agencies to help them improve the quality of care they provide.

---

<sup>13</sup>Abbreviated medical review was required for all episodes during most of the early months of the demonstration included in this analysis.

## II. DATA AND METHODOLOGY

We estimate program impacts on measures of home health cost per episode and cost per visit. To do this, we use primarily agency-level data on treatment-control group differences in the change in average costs between the predemonstration periods. We also estimate impacts on cost per episode using episode-level data for the demonstration period only. This analysis uses regression models to control for differences in patient characteristics, differences that may be confounded with the impacts of prospective payment.

Although our analysis focuses on agency responses to the demonstration incentives, we begin this chapter with a description of the construction of the data used in the supporting episode analyses. The reason we present the episode level data first is that the episode level variables are the building blocks of the agency level variables; most of the agency level variables are aggregations of the episode level variables. Thus, it is important to understand how the episode level variables are constructed before we discuss how they were aggregated to the agency level.

### A. DATA

#### 1. Constructing Episodes

To construct an episode of care at an individual episode level, we did the following:

- Identified the episode start and end dates using UB92 data.
- Matched the episode dates from the UB92 data to the Medicare Standard Analytical File to create the service use measures.
- Match the episodes to other Medicare files to obtain data for control variables.

The following sections describe how and why we did each step.

### **a. Identifying Episodes**

For patients of both treatment and control agencies, data from UB-92 bill record files obtained from the demonstration FI, PGBA, were used to identify home health episodes as defined by demonstration rules. We used UB92 data to define our episodes dates because the control variables on patient characteristics needed for the analysis are collected on the UB92. By using the UB92 to define the beginning date of the episode, we are assured that we have the patient characteristics that correspond to the episode date.

To construct episode "begin" and "end" dates, we started with each agency's enrollment in the demonstration, and scanned the UB-92 files to identify the first admission for each individual and all that person's subsequent bill records. We combined all records for an individual for 120 days following the first admission and any bills for care after 120 days until we observed a gap of at least 45 days in billing dates. This procedure was followed regardless of whether the agency discharged and readmitted a patient during the 165 (120 + 45) days. If we observed additional home health bills or if a patient was readmitted after a service gap of 45 days after the 120-day risk period, we created a second episode for that individual, and so on for any subsequent episodes beginning through August 31, 1996.

We chose the August 31 cut off date because any episodes which began after that are subject to incomplete data. The reason is as follows. The data used in this analysis was extracted from Medicare's Standard Analytic Files in May, 1997. Since claims are generally included in the standard analytic files within four months after the services were rendered, we should have nearly complete data on home health services received through December 1996. Because we want to analyze the agencies' behavior in the first 120 days of an episode (the at-risk period), we can only

include episodes that were complete by December, 1996. This establishes our cut-off date of August 31, 1996.

#### **b. Matching to Medicare Claims for Outcomes Variables**

Having established the beginning and end dates of the episode using the UB-92 data, we matched those episodes to HCFA's Medicare standard analytical files to obtain measures of service use. Matches were identified for 57,216 episodes; only 493 episodes (less than one percent) did not match. We used the standard analytical file data rather than the UB-92 data to construct service use measures because HCFA adjusts the standard analytical file data, but not the UB-92 data, for any voided or amended bills. Hence, it is a more accurate measure of the services HCFA pays for. In addition, because any measures of service use from the predemonstration period would use the Medicare standard analytical files, we needed to be sure that the same adjustment algorithms were applied in all periods.

Using standard analytical file data, we constructed measures of service use for each of the six Medicare-reimbursed services. Only Part A home health data were used for this analysis, since home health care provided under Part B ambulatory benefit is not eligible for per-episode payment.<sup>1</sup> In addition, we extracted claims only for the demonstration agency admitting the patient for a given episode. Impacts on services rendered by other home health agencies during the 120-day period were addressed in another report (Schore 1997).

---

<sup>1</sup> Patients who are ineligible for Part A Medicare benefits can receive home health services under Part B, and these visits are covered by the episode payment. Those visits rendered as ambulatory Part B visits are not covered by the episode payment. The data file, however, fail to distinguish between these two types of Part B services. Because Part B is such a small component of the total home health expenditures, we elected to exclude it.

### **c. Matching to Medicare Files for Control Variables**

Having constructed our home health outcomes, we then attached control variables to each episode. Control variables are used because we cannot be certain that random assignment produced treatment and control agencies that are exactly alike. In addition, by controlling for factors that will vary with the cost of care, we can increase the efficiency of our estimates.

As Table II.1 indicates, we used several types of control variables in this analysis. Patient characteristics at the start of an episode and patient Medicare service use in the six months preceding the episode were used to control for differences between the patients of treatment and control agencies. Agency and area characteristics were used to control for differences between the treatment and control agencies that might influence use of home health services.

**Patient Characteristics.** Patient characteristic control variables were used in the analysis to account for possible differences in patient mix between treatment and control agencies. We expect that individuals who are severely ill, have diagnoses requiring more care, and have greater limitations in daily activities will require more Medicare home health services.

We obtained data on patient characteristics at the start of the home health episode from three sources: (1) patient characteristic data collected in the UB-92 for the demonstration's case-mix adjuster, (2) Medicare enrollment databases, and (3) Medicare standard analytical files. In the remarks field for the first UB-92 bill following a demonstration admission, both treatment and control agencies were required to submit the information on patient characteristics needed for the 18-category Home Health Utilization Group (HHUG) case-mix adjuster. The characteristics include measures of impairment in Activities of Daily Living (ADLs) and whether the patient has certain medical conditions (cancer, diabetes, decubiti) or care needs (complex wound care). Medicare enrollment files provide us with basic patient demographic information, including the patient's age

TABLE II.1

## CONTROL VARIABLES FOR EPISODE-LEVEL MULTIVARIATE ANALYSIS, BY SOURCE

Episode Level		Agency Level		Area Level
Patient Characteristics at Episode Start (Medicare Enrollment Database; UB-92 remarks)	Medicare Service Use in Year Preceding Episode (Medicare Standard Analytical Files)	Base-Quarter Patient Service Use (Abt Base Quarter Case-Mix File)	Agency Characteristics (Cost Reports and Abt enrollment file)	Area Characteristics (Area Resource File)
Age	Length of pre-home health inpatient stay	Agency practice pattern index	Chain member	Physicians per 10,000 (1994)
Gender	Whether in SNF within 14 days before episode start		Hospital-based	Nursing home beds per 100 elderly residents (1991)
Race			Proprietary	
Original reason for entitlement	Number of home health visits in 6 months prior to episode start		Small agency (fewer than 30,000 visits in base year)	Hospital occupancy rate (1993)
Whether has cancer	Total Part A Medicare reimbursement in 6 months prior to episode start		State	
Whether has diabetes			Rural	
Whether has decubiti				
Needs complex wound care				
Limitations in activities of daily living				
Whether admitted to home health from hospital				

(at the start of home health episode), gender, race, and disability status (from the original reason for Medicare entitlement). From the standard analytical files, we constructed measures of Medicare service use to capture the patient's severity of illness, including measures of recent acute illness (whether admitted from hospital, length of prior hospital stay) and longer-term home health use (six months prior to admission). For the latter, we used the mean value for beneficiaries between 65.5 and 66 years old as a proxy measure for beneficiaries less than 65.5 years old at home health admission, since beneficiaries under age 65.5 would not have been eligible for Medicare service for a full six months.

**Agency and Area Baseline Characteristics.** Agency characteristics are used as control variables because different types of agencies may have different goals and different cost and management structures, which could affect the home health care they render. In addition, certain types of agencies may serve a mix of patients requiring more (or less) care than the patients of other agencies. For example, proprietary and nonprofit agencies might have different preexisting practice patterns with respect to the number of visits rendered per episode, and hospital-based agencies might serve a higher proportion of patients with acute illnesses than freestanding agencies do.

Agency characteristics are also used to define subgroups, because agencies with different goals, cost and management structures, and practice patterns could respond differently to the incentives of the demonstration. For example, proprietary agencies may have a stronger interest in revenue surpluses (profits) than nonprofit agencies and may therefore reduce visits by a greater margin.

Data on agencies' structural characteristics were obtained from base-year Medicare cost reports and from the demonstration implementation contractor, Abt Associates. Base-year Medicare cost reports provided information on the agencies' base-year characteristics, including for-profit status, affiliation, and size (as measured by total number of visits rendered). Measures that Abt Associates

constructed during the demonstration were used and included factors such as whether the agency was a member of a chain or was located in a rural area (according to the census definition).

We used case-mix data for the base quarter (also collected by Abt Associates) to develop a control variable measuring each agency's predemonstration practice pattern for the 120-day period. This practice pattern measure is an index of the average number of visits received by an agency's patients relative to the average number provided by other demonstration agencies. To control for differences in agency case mix in constructing the index, we use a weighted average of ratios in the 18 case-mix cells to determine the average number of visits. We first construct, for each case-mix category, the ratio of the agency's average number of visits of each type times the national cost limit to the same cost-weighted average for all agencies for this case-mix category. The ratio for a given category is weighted by the category's share of total episodes for the agency in the base quarter. An average ratio greater than one indicates that, controlling for differences in case mix, an agency provided more visits during the 120-day period than did other demonstration agencies during the quarter preceding the demonstration.

Area-level characteristics that might influence the care an agency renders were also controlled for. For example, in areas where the number of nursing home beds is limited (relative to demand), hospitals may discharge to home health care some patients who otherwise would be discharged to nursing home care. We obtained area characteristics from the ARF, including physicians per 10,000 residents, nursing home beds per 100 elderly residents, and hospital occupancy rates.

#### **d. Constructing the Analysis Sample**

The analysis sample contains only 38,832 episodes in the treatment year. The remaining episodes were dropped for the following reasons:



- Episodes that started outside the initial eight months of the demonstration year were excluded, for a number of reasons, including (1) we have cost data only for the first demonstration year; (2) for some agencies, there were only eight months of data; and (3) the data would be compared to predemonstration data, which include only eight months of episodes. (This accounts for 7,797 episodes.)
- From the initial set of 91 agencies, three agencies dropped out of the demonstration early because they were purchased by another agency or merged with another agency, and the new ownership did not want to be part of the demonstration. (This accounts for 1,613 episodes.) One agency did not have any episode admissions during the period; it is also excluded from the analysis.
- Five agencies failed to bill for demonstration services completely, which caused an undercount of service use. (This accounts for 3,459 episodes.)
- Two agencies did not provide cost reports from the first demonstration year, as will be discussed in the agency-level data section. (This accounts for 1,467 episodes.)

The agencies that were dropped for failure to bill completely warrant further explanation. During our previous analysis (Cheh et al. 1997), we discovered that some treatment agencies were not submitting bills for all services rendered during the episode. Because the treatment agencies were paid per episode and upon acceptance of the initial bill, these agencies did not believe it was in their best interest to incur billing costs for visits rendered later in the 120-day at-risk period.<sup>2</sup> Because this billing behavior was due to the agencies' treatment status and not a random management factor, the inclusion of agencies that did not completely submit all bills would make the impact estimates meaningless.

To measure the extent of this problem, we conducted two data analyses. The first examined bill records and checked for the frequency of "out-of-order" bills. Using episodes that were at least 60 days long, we identified those episodes that included only an initial bill and a discharge bill and

---

<sup>2</sup>Because of the complicated nature of the cost-reporting process and the calculation of losses, there is some financial advantage to bill completely. However, not all agencies understood the advantage at this point in the demonstration.

compared the proportion of these episodes across agencies. We found that many agencies had this type of billing pattern in 10 to 20 percent of their episodes in both the treatment and control agencies, suggesting that this was a common occurrence and could not be used effectively, by itself, to identify agencies that failed to submit interim bills because of the new payment system.

The second analysis used Medicare claims data and the quality assurance (QA) data. In this analysis, we compared the bill through date of the last Medicare bill of the episode with the last day of service as recorded on the QA forms. If agencies failed to submit all their bills, then the last day of service from the QA form would be after the discharge date obtained from the bills.

Two problems arose with this analysis. First, the QA data collection did not begin until May 1996--several months after the last agencies enrolled in the demonstration. As a result, the data available for the analysis are limited--only four months' worth of episodes. In addition, early in the demonstration, not all agencies submitted QA data systematically. As a result, we had QA data for only a small fraction of many agencies' episodes, which led us to question whether the episodes with QA data were representative of all episodes. In addition, we discovered that, for agencies using telephone contacts in lieu of visits, the last date of service (the telephone contact) may be after the discharge date (the last visit date). Because treatment agencies have the financial incentive to make telephone contacts, they would likely have higher frequencies of the last date of service following a discharge date.

Given the limitations of these analyses, we contacted all the agencies in the demonstration and interviewed their billing staff about their practices. We found:

- As a direct result of the new payment system, four treatment agencies were not billing for all visits. One agency had billing software that did not allow it to bill for visits that cost \$0. The other three agencies stated either that they didn't have the resources to submit bills that were not being paid and hence did not bother to submit the interim bills or that they intended to submit their bills but have not had the time.

- One agency billed for all visits, but if the claim was “kicked back” for any reason, only initial episode or outlier visits were resubmitted.
- Other agencies--12 treatment and 9 control--reported not billing for all visits for reasons unrelated to the demonstration. For example, many reported cases in which the local doctor had refused to sign the plan of treatment, making it impossible for the agency to bill Medicare for the service.

Comparing data from all three sources--bill records, QA, and interviews--we found nine agencies whose pattern of intermittent billing and mismatching discharge dates were suspiciously out of the ordinary. Of those nine agencies, eight told us that they had had trouble billing. The one agency that did not report trouble billing was very small and had few episodes, which may explain why its billing data appeared unusual. Of the eight agencies that admitted to having submitted incomplete billing, four indicated that the problems were related to the new payment system, and four indicated that the problems were unrelated. Finally, one agency admitted to having submitted incomplete bills but had not been identified as suspicious through the use of the other two secondary data sources; however, that agency had very few episodes and submitted no QA data.

Our decision was to exclude from the analysis the five agencies that had clearly stated that they failed to submit (or resubmit) bills in response to the new payment system. This included the four agencies that did not submit all bills and the one that did not resubmit “kicked-back” bills. Because the other billing problems were found among both the treatment and controls and were apparently random, we included all remaining agencies in the analysis.<sup>3</sup>

In addition to losing observations from dropping agencies, we dropped 1,067 episodes because we learned from the HCFA enrollment files that these patients were enrolled in a Medicare health maintenance organization (HMO) at the time of their home health admission and were not eligible

---

<sup>3</sup>In our conversations with billing clerks, many explained that, while they did not experience major problems at that time, no system was 100 percent foolproof.

to participate in the demonstration. The admitting demonstration agencies, probably unaware that these patients were HMO members when they admitted them, submitted a claim to the demonstration FI in error. We excluded another 1,211 episodes because Medicare was a secondary payer, and agencies were not entitled to a per-episode payment. During the demonstration, treatment and control agencies are both reimbursed at cost for care for which Medicare is a secondary payer.

Finally, we excluded 1,680 episodes that were missing the patient characteristic data from the remarks section of the UB-92. These data had been inadvertently erased from the UB-92s because of an error in the software used by PGBA, the demonstration FI. PGBA is working to restore the data but could not complete the task in time for this report.

We should note that this sample of episodes is substantially smaller than the one we used in the earlier preliminary report on the use of services (Cheh et al. 1997). The analysis samples differ for two reasons. First, since that report was submitted, we identified three additional agencies that had incomplete bill data--these agencies were dropped from this analysis. Second, in this analysis we had to match the episodes data to cost data; and because we only had cost data for the agency's first year in the demonstration, all episodes which began after the first year had to be excluded from the analysis.

#### **e. Summary Statistics for Episode Control Variables**

When the number of units randomized is not large, as is the case here (91 agencies), random assignment cannot be relied upon to yield treatment and control groups that are identical at baseline. Furthermore, as noted above, three agencies dropped out of the demonstration and eight were excluded from the analysis, which also could create differences between the two groups. Indeed, the two groups are not equivalent here: they differ on a number of observed characteristics.

Therefore, we cannot rely on simple treatment-control differences to produce unbiased estimates of program impacts; we must control for baseline differences in the treatment and control groups.

Table II.2 displays the treatment and control group means for the explanatory variables in our regression models. (Following the methodology described in Section B, these means have been constructed through the use of sample weights that give each agency equal representation.)<sup>4</sup>

With the large sample of episodes available, we have the statistical power to detect very small differences between the treatment and control groups at baseline.<sup>5</sup> We therefore expect that many differences in the explanatory variables between the treatment and control agencies will be statistically significant even when the magnitude of the difference is not large.

**Demographic Measures.** Some of the demographic characteristics of patients admitted to treatment and control agencies differed significantly, but the magnitudes of the differences are not large. Relative to control agencies, treatment agencies served a slightly higher proportion of patients age 85 and older and about three percent more patients originally entitled to Medicare because of age.

**Medical Conditions and Care.** There was one material difference between treatment and control agencies in the means for measures of medical conditions and care needs drawn from the case-mix data in the remarks field of the UB-92. Treatment agencies were significantly more likely

---

<sup>4</sup>The significance levels for the tests of equality between treatment and control group means in Table II.2 do not account for design effects due to the clustering. For our purposes here (but not in the main analysis described in Chapter IV), there is no need to account for these effects because we are interested only in differences within this sample, not the population of all agencies. We do account for the design effects associated with our use of sample weights, however. See Section B of this chapter for a complete discussion of the use of weighting and clustering in our analysis.

<sup>5</sup>For a binary indicator with a mean of 50 percent, for example, the minimum detectable difference is 1.3 percentage points, using a two-tailed test with a 95 percent confidence interval, at 80 percent power.

TABLE II.2

WEIGHTED MEANS FOR EXPLANATORY VARIABLES BY TREATMENT  
STATUS, AND TESTS FOR DIFFERENCES IN TREATMENT  
AND CONTROL GROUP MEANS

Explanatory Variable	Treatments	Controls
<b>Demographic Measures (Percentage)</b>		
Age Was Original Reason for Medicare Eligibility	83.1	80.8***
Age: Less than 65 Years Old	8.2	9.3**
Age: 75 to 84 Years Old	39.9	39.6
Age: Older than 84 Years	23.4	21.7**
Race: White	81.1	80.5
Female	63.5	64.5
<b>Medical Conditions and Care (Percentage)</b>		
Cancer	12.6	13.1
Diabetes	22.0	22.1
Cardiovascular Accident	14.9	14.9
Decubitus Ulcer: Stage 3 or 4	4.8	3.8**
Need Complex Wound Care	6.7	6.9
<b>Limitations in Activities of Daily Living (Percentage)</b>		
Bathing	71.7	73.6**
Eating	28.2	30.7***
Dressing	60.5	65.3***
Toileting	37.7	42.0***
Transferring	49.7	51.8***
<b>Patient Prior Service Use Measures</b>		
Admitted to Home Health from Hospital (Percentage)	33.7	37.9***
Length of Hospital Stay Ending Within 14 Days Before Home Health Start (Days)	3.4	4.2***
Whether Had a SNF Stay Ending Within 14 Days Before Home Health Start (Percentage)	17.2	14.7***

TABLE II.2 (continued)

Explanatory Variable	Treatments	Controls
Total Part A (Plus Part B IP/SNF/ Home Health/ Hospice) Reimbursement During 6 Months Before Home Health Start	\$10,798	\$11,261
Number of Part A Home Health Visits During 6 Months Before Home Health Start	12.7	13.8**
Had Medicare for Less than 6 Months at Episode Start (Percentage)	1.5	1.5
<b>Agency Characteristics Measures</b>		
Proprietary (Percentage)	47.5	52.5***
Hospital Based (Percentage)	5.0	12.5***
Chain (Percentage)	42.5	27.5***
Small Agency (Percent with Less than 30,000 Visits in Base Year)	35.0	20.0***
Agency Practice Pattern: Accounts for Varying Case Mixes Within Agencies	0.95	1.11***
<b>Area Characteristics Measures</b>		
State (Percentage)		
Florida	7.5	10.0***
Illinois	15.0	20.0***
Massachusetts	20.0	7.5***
Texas	35.0	40.0***
Urban	82.5	85.0
Number of Physicians per 10,000 Persons (1994)	21.98	21.28***
Number of Nursing Home Beds per 100 Persons over 65 (1991)	5.22	5.15
Hospital Occupancy Rate, 1993 (Percentage)	62.4	60.5***
<b>Sample Size</b>	<b>20,480</b>	<b>18,352</b>

\*Difference in means are significantly different from zero at the .10 level, two-tailed test.

\*\*Difference in means are significantly different from zero at the .05 level, two-tailed test.

\*\*\*Difference in means are significantly different from zero at the .01 level, two-tailed test.

to serve patients who had a decubitus ulcer, with a 24 percent increase in the treatment mean, relative to the control mean. However, the proportion of such cases was very small for both groups.

**Limitations in ADLs.** Relative to control agencies, treatment agencies served patients who were less likely to be impaired in ADLs, but the magnitudes of the differences are quite small. For all five ADL tasks for which we have measures (bathing, eating, dressing, toileting, and transferring), treatment agencies had significantly lower sample means than control agencies. However, all these treatment-control differences are less than 10 percent of the control group mean.

**Patient Prior Service Use Measures.** Patients served by treatment and control agencies differed somewhat in their use of medical services prior to entering a home health episode. Treatment agencies served patients who were more likely to have been recently discharged from a skilled nursing facility and less likely to have been admitted after release from a hospital. These differences offset one another: each is roughly two-and-a-half percentage points. In addition, prior hospital stays were shorter for patients in the treatment group than in the control group. For the treatment group, the mean length for a prior hospital stay was about 19 percent shorter than for the control group, and the number of home health visits was approximately 8 percent less.

**Agency Characteristics Measures.** There are several significant differences in the characteristics of treatment and control agencies, and a few of these differences are large. While treatment agencies are significantly less likely than control agencies to be hospital-based or proprietary, only the difference in the percent that is hospital-based is large, (with a difference of 60 percent from the control group mean.) On the other hand, treatment agencies are significantly more likely to be (1) affiliated with a chain, with a difference of 55 percent from the control group mean; and (2) small, with a difference of 15 percentage points, representing an increase of about 75 percent from the control group mean of 20 percent.



We also observe a material and statistically significant preexisting treatment-control difference in practice patterns. On average, treatment agencies delivered about 15 percent fewer visits per episode than the control group agencies, after adjustment for case-mix differences.

**Area Characteristics Measures.** Significant differences also exist between treatment and control agencies in area characteristics. The distribution across states differs for the treatment and control agencies, with the treatment group particularly overrepresented in Massachusetts and underrepresented in Illinois relative to the control group. There are also significant differences in the rate of physicians per 10,000 residents and the hospital occupancy rate; however, both of these differences are small, representing less than a three percent difference relative to the control group mean.

In summary, while the preexisting treatment-control differences in patient characteristics are minimal, there are several large differences in agency and area (state) characteristics. If these preexisting differences affect the provision of home health services, we would incorrectly estimate the effects of per-episode payment if we were to compare simple treatment and control group means. In fact, the large and extensive differences in agency and area characteristics suggest that a simple comparison of treatment and control group means could be very misleading. In the analysis, we employ regression-based models that control for these preexisting differences, thus providing more accurate estimates of the effect of the demonstration payment method. However, we cannot be completely certain that we controlled for all differences in baseline characteristics, which is why a differences-in-differences, or double-difference (DD), model approach can provide better estimates.

#### **f. Constructing Episodes for the Pre-demonstration Period**

To compare predemonstration service use with service use during the demonstration, we need to construct episodes for the period prior to the demonstration period. However, the UB92 data is

not available for the predemonstration period, making it necessary to use different data to construct the predemonstration episodes.

The measures of the predemonstration home health episode costs were constructed in the same manner as the demonstration year episodes, but with different data.<sup>6</sup> For each agency, we identified episodes using HCFA's standard analytical files and constructed an analytical file that included all episodes that began in the first eight months of the agency's fiscal year prior to the demonstration year. The period of the first eight months was chosen so that the episodes would all be completed within the predemonstration year, as the 120-day period episodes that begin after the eighth month would spill over into the demonstration year.

The process yielded 53,948 episodes in the base year. As with the demonstration-year analysis, we excluded (1) episodes for which Medicare was a secondary payer; (2) episodes that were covered by an HMO; and (3) episodes of those agencies that had dropped out of the demonstration, had missing cost reports, or had changed their billing practices in response to the demonstration payment system. This yielded a file with 44,716 episodes.

## **2. Aggregating Episode Data to Agency-Level Analysis**

Having constructed individual episodes, the next step is to aggregate these data to the agency level. We aggregated the episodes for the predemonstration period and the demonstration period in exactly the same way. First, we created an individual cost-per-episode by multiplying the number of visits of each of the six types during the 120-day period by the cost per visit of that type for the agency delivering the episode. We used base-year costs per visit to create the cost per episode for

---

<sup>6</sup>The only difference in the process is that we identified episodes using the standard analytical files. Note that in both the predemonstration and demonstration data, we used the standard analytical files for constructing the use measures.

the base year, and first-year costs per visit to create the cost per episode for the demonstration year. We summed the products across the six visit types. We then averaged these individual episode costs for each agency to obtain an agency-level cost per episode.

### **3. Cost Per Visit**

The Medicare cost reports filed by the 88 participating agencies were the primary data source for the predemonstration trend analysis on cost per visit and the impact analysis on cost per visit. (They were also used with the bill record data for the construction of the cost per episode.) To conduct the trend analysis, we requested cost reports for four years: the first year of demonstration operations and the three before the demonstration began, when both the treatment and control agencies were paid under the cost-reimbursement system. Our final data set, which contained all four years of data for most agencies, was missing information for some years for some agencies (Table II.3). To conduct the analysis of cost per visit, a subset of this data--just the baseline and demonstration year--was used.

The cost-per-visit analysis includes the total cost per visit for each of the six visit types and the administrative cost per visit. "Total" cost consists of all costs, including all direct and all overhead costs. Agencies that did not supply visits of a given type in a specific year did not report costs for that type in that year. Administrative cost per visit is defined as Medicare's proportion of capital, billing, and administrative costs, divided by the number of Medicare visits. This measure is useful in understanding whether the agencies are controlling their overhead expenditures and for directly identifying any impacts that might arise from economies of scale.

The first limitation on our analysis is that the data may not be 100 percent accurate, although the information from the Medicare cost reports on visits by type and total reimbursable costs is considered to be reasonably accurate. Agencies, FIs, and HCFA have established audit procedures

TABLE II.3  
NUMBER OF AGENCIES PROVIDING DATA, BY DEMONSTRATION YEAR

	Three Years Predemonstration	Two Years Predemonstration	Base Year	First Demonstration Year
Original Number of Recruited Agencies	91	91	91	91
Number of Participating Agencies at End of First Demonstration Year	88	88	88	88
Number of Agencies Providing a Medicare Cost Report	83	88	88	85
Number of Agencies Providing a Medicare Cost Report and Included in the Analysis	74	80	80	80
Number of Freestanding Agencies Providing a Medicare Cost Report in the Analysis	67	73	73	73
Number of Hospital-Based Agencies Providing a Medicare Cost Report in the Analysis	7	7	7	7

NOTE: Because of missing pages of cost reports or changes in services offered, the number of observations for each type of visit varies: 74 agencies submitted complete data for all 4 years on skilled nursing costs, 73 on home health aide costs, 70 on physical therapy costs, 57 on occupational therapy costs, 61 on speech therapy costs, and 70 on medical social worker costs.

to ensure the accuracy of these data items, which are central to the cost-reimbursement process. In addition, most of the base-year and all the first-demonstration-year cost reports used in this analysis were preliminary and subject to revision in the audit and dispute resolution process. Although we would prefer to have settled cost reports, this would require waiting for audits to be completed--a task that historically takes a number of years. In addition, because of HCFA's need to conduct a large audit of a nationwide sample of agencies to establish rates for the upcoming national prospective payment system, audits of the demonstration agencies may be delayed even further or may not happen at all. Thus the preliminary data used in our analyses is the best available at this time.

The second limitation on our analysis is the accounting practices underlying the Medicare cost reports. These practices may fail to capture the true resource costs associated with providing home health care. For example, one hospital-based agency reported an average cost for social worker visits of \$533 in one year, compared with an average cost of \$144 for two other years and \$0 for the other year. The \$533 cost reflects a large one-year administrative expense that drove all the visit costs for that agency well beyond its usual labor costs. This \$0 cost reflects the fact that, in that year, the agency delivered only a few social work visits, which were probably rendered by hospital social workers who did not bill the agency. As a result, the accounting cost of the visits to the agency was only \$0, not reflective of the value of the resources actually required to deliver the visits. This type of extreme situation is relatively uncommon in the data set, but it illustrates the need for caution in interpretations of the cost report data and the need to test the sensitivity of our results to such outliers.

#### 4. Outcome and Control Variables for Agency-Level Analysis

The cost-per-visit outcome variables for the six Medicare-reimbursed services are taken directly from the cost report. These unit costs are also used to compute agency-level cost per episode, as described in Section 2. The administrative cost per visit is calculated from column 10 of Section III of the cost report; we totaled the expenses for capital, billing, and administration and allocated a proportion of this administrative cost to Medicare by multiplying total administrative cost by the proportion of total direct-service costs attributable to Medicare-reimbursed services. We divided this product by the total number of Medicare visits to obtain the average administrative cost per visit.

Because we are using DD models that compare each agency to its own base period, we do not need to use control variables. The method eliminates the effects of preexisting differences between treatment and control by comparing the *change* in costs. However, the demonstration may affect per-visit and per-episode costs both directly and indirectly. That is, in addition to directly affecting these costs (for example, by encouraging staff to complete more visits per day), the demonstration may also affect agencies' volume of services and types of patients, which in turn may affect cost per visit and cost per episode. With models that include the changes in volume and changes in patient characteristics variables as control variables, the treatment status variable will measure only the direct effects of the payment method. By using models that exclude these variables, we estimate the total effects of the demonstration.

Changes in patient characteristics were used to control for possible differences in the types of patients the agencies serve. We expect that, if an agency serves a greater number of frail or elderly patients, then the cost per visit would increase, because the case is generally more complicated and requires longer visits.

We measured the changes in the agencies' patient characteristics as follows. First, we aggregated the individual episode characteristics to the agency level using the previously described episode databases for both the predemonstration year and the first demonstration year. We then subtracted the predemonstration year characteristics from the demonstration year characteristics to obtain the change in characteristics. For example, if 80 percent of an agencies episodes were for female patients in the predemonstration year, and 85 percent of the agencies episodes were for female patients in the first demonstration year, the change in the agency's percent of females would be measured as 5 percent (85-80).

Changes in agencies' volume were used to control for possible cost changes due to economies of scale. We expect that, if an agency decreases in size, then the per-visit cost will increase. We measured the change in agency visits by subtracting the number of visits rendered in the base year from the number of visits rendered in the first year of the demonstration.

#### **a. Analysis Sample**

As noted earlier, 88 agencies were participating in the demonstration in the first year. Our analysis sample includes the same 40 treatment and 40 control agencies used in the episode sample. As with the episode analysis, we excluded from the cost-per-visit analysis five agencies that failed to submit complete bills in response to the demonstration payment system. This was necessary because the number of visits reported on an agency's cost report must correspond to the number of visits billed to Medicare. (If agencies fail to bill for all visits, the cost per visit will be artificially high.) In addition, we excluded two agencies that failed to submit demonstration year cost reports.<sup>7</sup>

---

<sup>7</sup>Note that three agencies failed to submit a cost report, but one of those agencies also failed to submit complete bills.

Finally, we dropped one treatment agency that did not bill for any episodes in the first demonstration year. Since the agency did not have any patients who were paid under the prospective payment system, it was not sensible to use this sample point to measure the intervention's effect on cost per visit.

#### **b. Summary Statistics for Control Variables**

As noted earlier, when the number randomized is not large, random assignment cannot be relied upon to yield treatment and control groups that are identical at baseline. Furthermore, the number of agencies that were excluded from the analysis could create differences between the two groups. The methodology used in the analysis largely eliminates the effects of preexisting differences by comparing the two groups on the change in costs between the predemonstration and demonstration periods. However, in an effort to distinguish the direct and indirect effects of the intervention on cost per visit, we included control variables for the change in patient mix volume in some analyses.

Table II.4 displays the treatment and control group means for the explanatory variables used in our regression models. Since these measures may be affected by the demonstration, we expect that the treatment and control means may differ. As Table II.4 shows, there were large differences in the change in volume of visits between treatment and control agencies, but the changes in patient characteristics were almost identical for the two groups.

### **B. ANALYTICAL METHODS**

Since the focus of our analysis is on the agency-level response to the per-episode demonstration, we generally make the agency the unit of observation and employ DD models to compute demonstration impacts. The DD model calculates the demonstration impact as the mean difference between treatment and control agencies in the change in outcomes between the predemonstration and



TABLE II.4  
MEAN VALUES OF EXPLANATORY VARIABLES USED IN AGENCY-LEVEL ANALYSIS

Explanatory Variable	Demonstration Period		Predemonstration Period		Change	
	Treatment	Control	Treatment	Control	Treatment	Control
<b>Patient Characteristic</b>						
Proportion of Non-Minority Episodes	.81	.81	.83	.82	.02	.01
Proportion of Female Episodes	.64	.65	.63	.66	-.01	-.01
Proportion of Beneficiaries Who Originally Received Medicare Because of Their Age	.83	.81	.83	.82	.00	-.01
Proportion of Episodes for Beneficiaries Less than 65 Years Old	.08	.09	.09	.10	.01	.01
Proportion of Episodes for Beneficiaries Between 75 and 84 Years Old	.40	.40	.39	.41	-.01	.01
Proportion of Episodes for Beneficiaries 85 Years Old or Older	.24	.22	.25	.22	.01	.00
<b>Volume Measures</b>						
Total Visits	95,530	73,029	110,091	79,480	-14,561	-6,451*
Skilled Nursing Visits	36,925	33,463	44,035	37,506	-7,110	-4,043**
Physical Therapy Visits	6,935	7,037	8,044	7,688	-1,109	-651
Occupational Therapy Visits	1,127	7,360	2,537	1,410	-298	-50**
Speech Visits	447	417	599	483	-152	-66
Medical Social Worker Visits	716	971	860	1,039	-144	-68
Change in Home Health Aide Visits	48,232	29,781	53,981	32,330	-5,749	-2,549
Sample Size	40	40	40	40	40	40

SOURCE: Medicare Bill Records and Cost Reports.

\*Significantly different from zero at the .10 level, two-tailed test.

\*\*Significantly different from zero at the .05 level, two-tailed test.

\*\*\*Significantly different from zero at the .01 level, two-tailed test.

demonstration periods. For example, assume that the average change in cost per episode from predemonstration to demonstration was \$200 for treatment agencies and \$400 for control agencies. Since the only factor that differed between treatment and control agencies over this time period was their form of payment, the difference in the cost-per-episode growth, -\$200, reflects the (average) impact of the demonstration among participating agencies.

We also conduct analyses in which the episode of care is the unit of observation and use standard ordinary least squares (OLS) regression models to examine treatment impacts using (only) demonstration period data. A principal benefit of this "levels" model is that we use as control variables all the detailed patient data that were gathered at admissions. Thus, we are better able to examine the sensitivity of our impact estimates to important factors such as patient case mix. In addition, the much larger number of observations available on episodes than agencies may substantially improve the precision of the impact estimates for these outcomes, particularly for subgroups that may include only a handful of agencies.<sup>8</sup> Despite these benefits, however, the levels model is less desirable than the DD approach, because it can control only for those patient and agency-level characteristics that we observe. Thus, the estimates from this model may be subject to far more statistical bias than the DD model, which controls for *all* fixed factors through taking the difference of outcomes between periods.

---

<sup>8</sup>The gain in sample size will be offset to some extent by the design effects associated with weighting and clustering of observations within agencies. This issue is discussed in detail later in this section.

## 1. Agency-Level Analysis: DD Models

Operationally, the basic (or unadjusted) DD model is identical to an OLS regression where the dependent variable is the agency-level change in a given outcome between the predemonstration and the demonstration and the regressors are a constant term and each agency's treatment status:

$$(1) \quad \Delta Y = \alpha + \delta T + \epsilon,$$

where:

$\Delta Y$  is the demonstration period value of the outcome of interest (cost per episode, cost per visit, etc.) minus the predemonstration period value

$T$  is a binary variable for treatment status that equals 1 for episodes taking place in treatment agencies and 0 for episodes taking place in control agencies

$\alpha$  is the constant term

$\delta$  is a parameter that measures the impact of prospective rate setting on the outcome  $Y$

$\epsilon$  is a random disturbance term assumed to have a mean of zero (conditional on  $T$  and  $\alpha$ ) that reflects all of the unobserved factors affecting  $\Delta Y$

The key parameter in the model is the coefficient  $\delta$ , which provides the estimated effect of the demonstration on the outcome of interest.<sup>9</sup>

The essential statistical assumption underlying equation (1) is that no unobserved factors correlated with an agency's demonstration status also affect the outcome of interest. If this assumption is violated, the estimated demonstration impact from this simple DD specification may be biased. Fortunately, two factors greatly reduce the possibility of such bias. The first is the

---

<sup>9</sup>Recall that  $\delta$  measures this impact as the average change made by the treatment from the predemonstration to the demonstration minus the average change made by the control agencies.

random assignment of agencies into treatment and control groups, which greatly reduces the likelihood of unobservables being correlated with treatment status. The second is the specification of the model (where the dependent variable is the change through time), which nets out the influence of any fixed factors that might be correlated with treatment status.

A principal weakness of the simple DD model is that it cannot account for factors that change through time. If such factors are correlated with agencies' treatment status and have a significant impact on our outcome of interest, our estimated demonstration impacts may be biased. Of the factors that may change over time, the most important are the characteristics of patients and the volume of services that agencies provide. If we incorporate these factors into equation (1), we acquire the following "regression-adjusted" DD model:

$$(2) \quad \Delta Y = \alpha + (\Delta X)B + \delta^*T + \epsilon,$$

where:

$\Delta Y$  is the demonstration period value of the outcome of interest (cost per episode, cost per visit, etc.) minus the predemonstration period value

$\Delta X$  is a vector of patient and/or agency level characteristics (for example, volume) that may change over time

$\alpha$  is the constant term

$B$  is the vector of regression coefficients on the control variables

$\delta^*$  is a parameter that measures the impact of prospective rate setting on the outcome  $Y$

$\epsilon$  is a random disturbance term assumed to have a mean of zero (conditional on  $T$  and  $\alpha$ ) that reflects all of the unobserved factors affecting  $\Delta Y$

The coefficient  $\delta^*$  again provides the estimate of the demonstration impact on the outcome of interest.

Because the demonstration may *directly* affect both patient characteristics and service volume, differences in estimated demonstration impacts when controlling and not controlling for these factors do not necessarily suggest that our “unadjusted” impacts are biased. Instead, they may indicate that the demonstration impact is operating indirectly through its influence on patient mix or service volume. For example, assume that the upward impact of the demonstration on cost per visit is halved when controlling for change in service volume. While this may suggest that the estimate from the unadjusted model is biased, it may also suggest that much of the rise in cost per visit resulting from the demonstration is due to lost economies of scale as treatment agencies reduced visits per episode. To sort out what is estimation bias and what is an indirect demonstration effect, one can estimate the model given by equation (1) using the change in volume and patient mix as dependent variables. All else equal, the larger the impact of the demonstration on these variables, the more the change in the direct impact ( $\delta^* - \delta$ ) may be attributed to indirect demonstration effects operating through changes in volume and patient mix.

An additional weakness of the simple DD model is that it does not allow for the possibility that the *change* in the outcome of interest may be different for agencies depending on their fixed characteristics. If these characteristics are also correlated with agencies’ treatment status, our estimates of demonstration impacts may be biased. In principle, controlling for such factors is straightforward: the regression-adjusted DD model shown in equation (2) is simply refined to include a set of fixed agency characteristics as control variables. However, this model itself is limited, because it cannot determine whether agencies with different characteristics have different levels of change in outcomes in general (for example, for-profit plans exhibit greater changes in cost per visit than nonprofits) or in response to the demonstration (for example, for-profit treatment agencies respond more strongly to the demonstration but would otherwise have the same time trend

as nonprofits). If they are changing differently only in response to the demonstration, our estimates under the simple DD model are unbiased, while those under the regression-adjusted model may be biased. While this ambiguity could be resolved by adding interaction terms involving treatment status and the subgroup to Model 2, along with the subgroup variables, the number of degrees of freedom can become quite small with only two years of data. We regard this type of bias to be relatively unlikely; however, we will explain it more thoroughly in the final report, when data for more years are available.

## 2. Episode-Level Analysis: Levels Regression Models

One general limitation of the DD approach is that any included control variables must be available for both the predemonstration and the demonstration periods. Our patient characteristics data from the predemonstration is limited to basic demographic information, which brings into question whether our regression-adjusted DD models are able to examine thoroughly the sensitivity of our estimates to changes in agencies' case mix. In addition, as noted previously, the DD model effectively ignores the within-agency sample that exists for episode-based outcomes, leading to reduced precision in these estimates.

Given these shortcomings, we also estimate a set of levels (one-period) regression models using episode data from only the demonstration period. The outcomes that we examine through these models are only those that vary by episode: the cost per episode and visits per episode. The basic structure of the model is as follows:

$$(3) \quad Y = \alpha + XB + \delta^*T + \epsilon,$$

where:

$Y$  is an outcome (number of visits, cost per episode) for episodes taking place during the demonstration period

$X$  is a vector of control variables reflecting agency and patient-level characteristics

$T$  is a binary variable for treatment status that equals 1 for episodes taking place in treatment agencies and 0 for episodes taking place in control agencies

$\alpha$  is the intercept term

$B$  is the vector of regression coefficients on the control variables

$\delta^{**}$  is a parameter that measures the impact of prospective rate-setting on the outcome  $Y$

$\epsilon$  is a random disturbance term assumed to have a mean of zero (conditional on  $X$ ,  $T$ , and  $\alpha$ ) that reflects all of the unobserved factors affecting  $Y$

The coefficient  $\delta^{**}$  provides the estimated demonstration impact on the outcome  $Y$ .

As noted previously, the inability of the "levels" model to control for fixed factors that may be unobserved (agency-specific behavior, local economic conditions) makes it far more prone to statistical bias. Thus, we estimate these models primarily as a test of robustness of the main findings from the DD models.

**Weighting.** Because the intervention is conducted at the agency level, we weight the individual episodes to give agencies equal representation in the analysis. For each agency, we construct this "agency equal" weight as follows:

$$(4) \quad w_i = \frac{1/n_i}{k/n},$$

where  $n_i$  is the number of (episode-level) observations,  $k$  is the number of agencies, and  $n$  is the total number of observations for all agencies. All episodes delivered by agency  $i$  received weight  $w_i$  in the analysis.

**Design Effects.** The episode-level sample consists of patient episodes that have been clustered in a relatively small number of agencies. The variances of the impact estimates generated from standard statistical packages account for the sampling of patients admitted to demonstration agencies but do not account for the sampling of agencies that participated in the demonstration.<sup>10</sup> Thus, the significance levels of the impact estimates reported by standard statistical packages can be used only to make internally valid statements about the effect of the per-episode rate setting in the demonstration agencies.

The objective of the evaluation, however, is to generalize program estimates to all home health agencies so that externally valid statements can be made about likely program effects if prospective payment is implemented nationwide. To do this, we adjust the variance of the impact estimates using SUDAAN software that accounts for the design effects associated with the clustering of patients within agencies in the demonstration.<sup>11,12</sup> Thus, we account for both the variance in (episodic) outcomes within each agency and the variance in mean outcomes between agencies.

### 3. Subgroup Analysis

We investigate demonstration impacts for agency subgroups for two reasons. First, to the extent that we find such impacts of per-episode payment, we are interested in whether they differ between

---

<sup>10</sup>Our sample actually includes the population of patient episodes taking place in demonstration agencies over the demonstration period under investigation. However, because we wish to make inferences about the outcomes for patients admitted through time, we treat these episodes as a simple random sample from the pool of all (future) episodes.

<sup>11</sup>The design effect calculations assume that the agencies were selected at random first, then patients were selected at random from each agency. Neither assumption is true, but the calculations do give us the correct variance that results from selecting patients for the study in two stages and not including the set of all possible agencies.

<sup>12</sup>The design effect calculation also accounts for the greater variance (statistical inefficiency) introduced by using sample weights in the regression models.



subgroups of agencies defined by their profit status and other important characteristics. If we find no overall impacts, we wish to determine whether they exist for selected subgroups.

When DD models are used, the analysis of agency subgroups is performed by estimating the following regression model:

$$(5) \quad \Delta Y = \alpha + \lambda T + \sum_{i=1}^n \phi_i S_i + \sum_{i=1}^n \gamma_i (T * S_i) + \epsilon,$$

where:

$\Delta Y$  is the demonstration period value of the outcome of interest (cost per episode, cost per visit, etc.) minus the predemonstration period value

$T$  is a binary variable for treatment status that equals 1 for episodes taking place in treatment agencies and 0 for episodes taking place in control agencies

$S_i$  is an indicator variable for the  $i$ th subgroup, which equals 1 if the agency has the characteristic, and zero otherwise

$\alpha$  is the constant term

$\lambda$  is a parameter that measures the impact of prospective rate setting on  $Y$  among agencies without characteristic(s)  $i$ ;  $i = 1$  to  $n$ .

$\phi_i$  is a parameter that measures the effect of being an agency with and without the  $i$ th characteristic

$\gamma_i$  is a parameter that measures the difference in the impact of prospective rate setting on the outcome measure between agencies with and without the  $i$ th characteristic

$\epsilon$  is a random disturbance term assumed to have a mean of zero (conditional on  $T$  and  $\alpha$ ) that reflects all of the unobserved factors affecting  $\Delta Y$

Because of the limited number of observations (agencies) available under the DD approach, we typically conduct the subgroup analysis by estimating equation (1) for one subgroup at a time ( $n=1$ ).

We assess whether the demonstration impacts differ between particular subgroups by examining the statistical significance of the individual interaction term  $\gamma_i$ . For example, suppose that the p-value on the interaction term between an agency's for-profit status and its treatment status is .04 (statistically significant from zero). This indicates that for the selected outcome of interest, the effect of prospective payment differs significantly between for-profit and nonprofit agencies.

We also test whether the effect of the payment method is significantly different from zero for each subgroup.<sup>13</sup> We conduct the test by setting the indicator variable for the subgroup appropriately ( $s_i = 1$  if agency  $i$  has the  $i$ th characteristic; 0 otherwise), and then examine the statistical significance of the treatment parameter,  $\lambda + \gamma_i$  (if  $s = 1$ ).

The drawback to this approach is that if subgroups are highly correlated, one may draw incorrect inferences about the size or significance of subgroup effects. For example, if most large agencies are also for-profit, one may find between large and small agencies a significant difference that is actually attributable to differences in proprietary status, not agency size. Given this limitation, we also examine selected subgroup impacts through a variation of the levels model described earlier:

$$(6) \quad Y = \alpha + XB + \lambda'T + \sum_{i=1}^n S_i \phi_i + \sum_{i=1}^n \gamma_i (T * S_i) + \epsilon.$$

The benefit of this model is that the larger sample size allows us to include all the subgroup interactions in a single regression, and to estimate impacts for subgroups defined by patient

---

<sup>13</sup>It may not be possible to identify relatively small impacts for subgroups, since tests of statistical significance lose power as the sample size declines. The smaller the size of the subgroup (all else equal), the less likely we are to reject the hypothesis that the payment method has no effect, for a true effect of any size.

characteristics. To the extent that the results are consistent with any subgroup differences found through the DD model, it strongly supports the existence of such differences.

#### 4. Hypothesis Tests for the Impact Estimates

For each of the outcomes that we investigate, a t-statistic tests the null hypothesis that there is no difference between the (regression-adjusted) means for treatment and control agencies.<sup>14</sup> For example, when we determine if prospective payment had an impact on the cost per episode, we are really testing whether the mean change in the cost per episode for the treatment agencies was different than the mean change in the cost per episode for the control agencies. The associated p-value is used to determine whether the demonstration had a measurable impact. A p-value below 0.10 indicates rejection of the null hypothesis and provides significant statistical evidence that a demonstration impact exists. At this p-value, however, approximately 10 percent of independent tests will be significant when there is no true difference (known as Type I error). Therefore, in assigning validity to any statistically significant impact, we are careful to consider whether the sign and magnitude of the predicted effect are consistent with related outcomes.

**Power of the Analysis.** Despite the relatively large number of agencies in the demonstration, the statistical precision provided by the DD models makes it likely that we would detect only fairly large demonstration impacts on cost per episode and other outcomes at the agency level. For example, the minimum detectable effect of the demonstration on the cost per episode in the agency-level analysis is about 12 percent of the base-year mean for all agencies under a two-tailed test at the 10 percent significance level, with 80 percent power. Based on the growth in control mean costs,

---

<sup>14</sup>For the subgroup impacts, the hypothesis test often involves a linear combination of variables because of the existence of interaction terms on the treatment status. In these cases, we use an F-test to test for demonstration impacts.

this reflects a growth in cost per episode for treatment agencies of about \$510 between the predemonstration and demonstration periods.

For the episode-level analyses, our large sample of patient episodes makes it more likely that we will identify demonstration impacts as statistically significant even after accounting for the design effects associated with clustering and weighting. For example, accounting for design effects, the minimum detectable effect of the demonstration on cost per episode in the episode-level analysis is about 9 percent under a two-tailed test at the 10 percent significance level, with 80 percent power.

## **5. Robustness**

While we expect the regression models used in the main analysis to be robust, they may be sensitive to two important factors. First, the general existence of outliers, either within or across agencies, may have undue influence on our findings. Second, as noted previously, a number of agencies have experienced difficulty submitting bills during the initial months of the demonstration. While the agencies remaining in our analysis have confirmed that such difficulties are unrelated to the payment method, we would like to confirm that billing problems are not affecting our principal results.

The method that we use to investigate the effects of outliers is to reestimate the models without the agencies with the two highest and two lowest mean values in the control group and the treatment group for a given outcome. The removal of these (eight) outlier agencies leaves 90 percent of the sample intact. To the extent that the results using this “trimmed” sample are similar to those using the full sample, our impact estimates are robust to the influence of the most extreme outliers.

To examine the effects of billing problems on demonstration impacts, we add a control variable in the DD models that reflects whether or not an agency reported problems with billing during the

first eight months of the demonstration.<sup>15</sup> Like the outlier analysis, our impact estimates are robust to the existence of billing problems if there is little or no change in the estimates under this alternative specification.

---

<sup>15</sup>Operationally, this is done by estimating the unadjusted DD model given by equation (1) including an additional explanatory (dummy) variable for misbilling.

### **III. CHARACTERISTICS AND TRENDS OF THE AGENCIES PARTICIPATING IN THE DEMONSTRATION**

In this chapter, we describe the characteristics of the agencies in the demonstration and their trends in service provision prior to entering the demonstration. The objective is to promote, by providing background information for the results in the ensuing chapters, understanding of the types of agencies enrolled in the demonstration and the issues these agencies faced at the time they enrolled. In future reports, we will examine how the characteristics of agencies compare with all Medicare-certified agencies and what the implications for national estimates are.

#### **A. WHAT TYPES OF AGENCIES ENROLLED IN THE DEMONSTRATION?**

Agency response to the demonstration's incentives may vary greatly by agency characteristics, making it important to have a mixture of agency types in the demonstration. For example, for-profit agencies may be more responsive to the profit motive than nonprofit agencies, whereas hospital-based agencies may be less responsive because they take the hospital's financial interests into account.

The agencies in the demonstration vary on several key characteristics, including size, auspice, and environment (see Table III.1). The average agency in the demonstration provided 89,650 visits in the base year, but the size ranged from just 4,393 to 1.3 million visits. Almost half the agencies were proprietary, and most (91 percent) were freestanding. The agencies came from all five demonstration states, but 60 percent of the agencies were located in Texas and California, reflecting the large number of Medicare home health agencies in those states. Eighty-four percent of the agencies were located in urban areas.

TABLE III.1

## AGENCY CHARACTERISTICS AT THE BEGINNING OF THE DEMONSTRATION

	Number	Percentage
State		
California	18	22.5
Florida	7	8.8
Illinois	14	17.5
Massachusetts	11	13.8
Texas	30	37.5
Urban/Rural		
Urban	67	83.8
Rural	13	16.3
Type		
Proprietary	40	50.0
Nonprofit	40	50.0
Freestanding	73	91.3
Hospital-based	7	8.8
Member of chain	28	35.0
Unaffiliated	52	65.0
Size		
Below 30,000 visits per year	22	27.5
Above 30,000 visits per year	58	72.5

SOURCE: Medicare Cost Reports.

## **B. WHAT WERE THE TRENDS PRIOR TO ENROLLMENT IN THE DEMONSTRATION?**

The home health prospective payment demonstration is taking place in a turbulent health care market. To put the impacts of prospective payment into perspective, it is important to understand how business was changing for agencies before they entered the demonstration.

Demonstration agencies were growing at a rapid pace as measured by the average number of visits rendered. From fiscal year 1993 to 1995, average total visits grew almost 31 percent, from 75,000 visits per agency to 98,000 (see Table III.2). Sixty-five percent of the agencies grew by 25 percent or more during this two-year period, and 79 percent grew by 10 percent or more. The Medicare program accounted for this entire increase in visits; the average number of Medicare visits rose from 64,000 to 87,000.

Much of this growth in services was due to an increase in visits per beneficiary. While the average number of visits grew 30 percent, the average number of Medicare beneficiaries served grew only 3 percent.<sup>1</sup> This increase in visits per patient reflects a number of different factors, including shorter hospital stays, increasing numbers of frail elderly beneficiaries, and increasing reliance on Medicare home health to address chronic care needs.

The large growth in the number of visits was accompanied by a substantial growth in staff. The total number of full-time-equivalent employees grew 26 percent from fiscal year 1993 to 1995, just slightly less than the 30 percent visit growth rate (Figure III.1). The growth was due to an increase in staff hired directly by the agencies--the number of contracted staff actually remained the same. This suggests that agencies viewed the increases in volume prior to the

---

<sup>1</sup>The number of Medicare beneficiaries is measured from the unduplicated census count from the Medicare Cost Reports. Anecdotal evidence indicates that some agencies report admissions rather than beneficiaries as the unduplicated census count.



TABLE III.2  
TRENDS IN THE VOLUME OF HOME HEALTH VISITS PER AGENCY

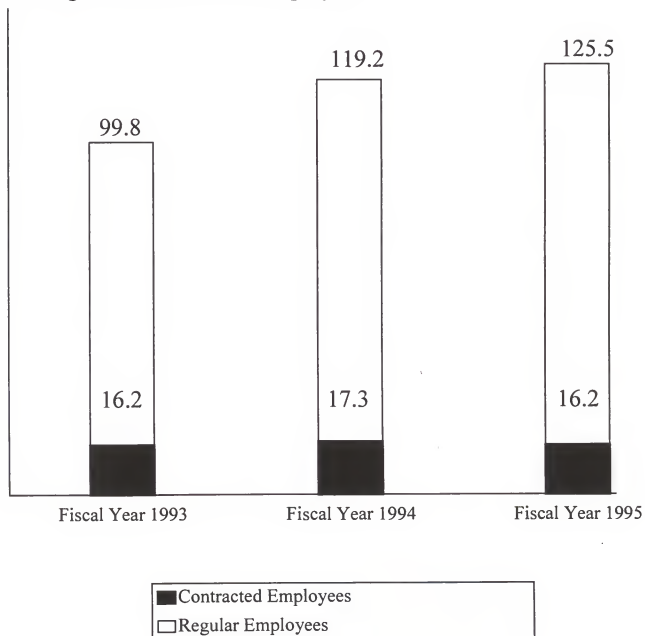
	FY 1993	FY 1994	FY 1995
<b>Volume of Services (Mean)</b>			
Total Agency Visits	75,055	89,657	97,810
Total Medicare Visits	64,314	78,557	86,875
<b>Volume of Patients (Mean)</b>			
Total Agency Patients	1,827	1,870	2,051
Total Medicare Patients	1,366	1,383	1,459

SOURCE: Medicare Cost Reports. Excludes agencies that did not submit all three years of costs reports. N = 75.

FIGURE III.1

TRENDS IN STAFFING LEVELS

Average Number of FTE Employees



Source: Medicare Cost Reports, N = 64 (some agencies failed to report employment statistics).

demonstration as permanent rather than transitory. Our early site visits confirmed this: despite the advent of the demonstration and increased managed care penetration, most of the agencies expected to continue to increase their visit volume during the demonstration.<sup>2</sup>

The slightly slower growth in full-time-equivalent staff levels, compared with the growth in visits suggests that agencies were increasing productivity in this period. During our early site visits, agencies acknowledged that productivity had increased and attributed it to two main factors. First, managed care organizations were starting to enroll significant numbers of patients in the area, and these managed care organizations bargained hard on payment rates. Thus, even agencies that were significantly below the Medicare cost limits were becoming increasingly cost conscious. Second, some agencies participated in their state Medicaid program, which paid a lower rate than the Medicare program. To remain solvent, these agencies had to keep their costs per visit either below the Medicaid amount or close enough so that other sources of revenue could make up the difference between Medicaid payments and actual costs.

The increase in productivity limited the growth in cost per visit for skilled nursing and home health aide services--the predominant types of services rendered under the home health benefit (see Table III.3). During these three years, the average cost for a skilled nursing visit for the demonstration agencies increased three percent, and the average cost for a home health aide visit increased one percent--both below the five percent annual inflation rate over this time.<sup>3</sup> During the site visits, agencies reported that they had instituted a number of cost containment efforts in the

---

<sup>2</sup>It is interesting to note that, while the overall number of contracted staff decreased slightly, the number of contracted home health aides increased but was offset by decreases in contracted nurses and physical therapists.

<sup>3</sup>The Gross Domestic Product Implicit Price Deflator increased five percent from 1992 to 1995.

TABLE III.3  
TRENDS IN AVERAGE COST PER VISIT  
(in Dollars)

	Fiscal Year 1993	Fiscal Year 1994	Fiscal Year 1995
<b>Average Cost Per Visit</b>			
Skilled Nursing	83.08	84.05	85.90
Physical Therapy	88.39	93.51	99.01
Occupational Therapy	88.09	93.63	93.71
Speech Therapy	89.46	101.30	96.39
Medical Social Services	127.09	133.25	155.04
Home Health Aide	37.69	37.67	38.11

SOURCE: Medicare Cost Reports, n = 74. (Includes only agencies with three years of data. Note that some agencies did not offer physical therapy, occupational therapy, speech therapy, and medical social worker services in all three years.)

period prior to entering the demonstration (Phillips and Thompson 1997). These data suggest that these efforts were effective.

While agencies were able to control skilled nursing and home health aide costs, market forces kept them from successfully controlling the costs of other Medicare-reimbursed services. Physical therapy cost per visit rose 22 percent, medical social worker cost per visit rose 14 percent, and occupational therapy cost per visit rose 11 percent. The large cost increases reflect both market increases in salaries for these providers and the initial introduction of these services by some agencies.<sup>4</sup>

In summary, the agencies enrolled in the demonstration typified the problem that HCFA is trying to solve with the per-episode prospective payment system. The demonstration agencies were holding down their unit costs for some services--but the volume of Medicare visits was escalating at an alarming rate. For the prospective payment system to be an effective Medicare cost containment strategy, the payment system must be able to induce resource savings among these types of agencies.

---

<sup>4</sup>In many cases when a new service is introduced, low volume may cause the service to have extraordinarily high costs for a period of time. Even if we eliminate agencies that started these services from the analysis, per-visit costs for these disciplines still grew faster than the rate of inflation.

#### IV. IMPACTS ON COST PER EPISODE IN THE FIRST YEAR

The primary way for agencies to generate profits in the per-episode payment demonstration is to lower their cost per episode relative to their base year. This could be accomplished through two methods: (1) reducing the cost per visit, or (2) reducing the number of visits per episode of care.

Agencies could decrease the cost per visit either by improving staff productivity or using less expensive methods to get the work done. Agencies could enhance productivity by, for example, paying field staff a per-visit rate instead of a salary, adopting cost-saving technology (such as voice mail), or using clerical staff to complete parts of the patient documentation. They could also reduce travel costs by reorganizing staff or increase their volume to take advantage of economies of scale. All these options are available to them under Medicare's cost-reimbursement system, but agencies have no incentive to adopt them under that system unless they are near the cost limits.

Agencies can reduce the number of visits per episode by (1) providing fewer visits per week without reducing the length of the episode, or (2) eliminating visits near the end of care and discharging the patient earlier. In either case, treatment agencies might seek care from patients' relatives, home- and community-based services, or another home health agency to substitute for visits they were no longer providing. In addition, for patients who are discharged earlier, hospice care or nursing home care might substitute for home health care. Treatment agencies might also eliminate visits in favor of telephone monitoring.

Treatment agencies could also reduce their visits (and thus cost) per episode by seeking a mix of patients needing less care than the patients they had served in the past. However, in the early site visits, we found no evidence that the treatment agencies did so. Rather, agencies reported that to maintain a flow of new business in an extremely competitive environment, they must be highly

responsive to the needs of their referral sources. Moreover, even if a demonstration agency was able to attract a mix of patients with less costly care needs than the ones previously served, it would not necessarily make larger profits, because the case-mix adjuster is intended to reduce payments to the agency in such a situation. This provides a further drawback to seeking profits by altering case-mix.

Site visits to the agencies during this time suggested that both treatment and control agencies were pursuing strategies to control per-visit costs in equal proportions (Phillips and Thompson 1997). When asked why they were trying to reduce their per-visit costs, few treatment agencies indicated that they were trying to cut cost per visit in response to the demonstration. Instead, most cited the same reason as the control group agencies: payers other than the Medicare program were pressuring them to keep costs low.

Treatment agencies, but not controls, reported implementing a number of different activities to reduce visits in response to the demonstration payment incentives. Some of these activities, such as greater supervision of care planning and greater use of care maps, could reduce either the frequency of visits or lead to an earlier discharge. Other activities, such as using specialists to speed the healing of wounds, are likely to reduce the length of the episode.

#### **A. DID PROSPECTIVE PAYMENT AFFECT THE COST PER EPISODE?**

The demonstration payment method gives agencies a clear incentive to reduce the costs of care during the first 120 days of care. If prospectively paid (treatment) agencies can reduce costs, they will make a profit. Control agencies have no financial incentive to cut cost per episode under the Medicare program (except the limited incentive to keep cost per visit below the limits).

The new payment system significantly decreased the cost per episode, even at this early period of the demonstration. Based on the DD models shown in Table IV.1, treatment agencies' cost per episode declined \$280 from the base year to the first demonstration year, while control agencies'

episode costs rose \$139. Thus, the average demonstration impact at the agency level was \$419 per episode, which represents a 13 percent savings over the expected cost for treatment agencies in absence of the demonstration ( $\$3,170 = \$3,031 + \$139$ ).

These results suggest that most agencies in both the treatment and control groups responded to their respective financial incentives. Fifty-eight percent of the treatment agencies decreased their cost per episode by five percent or more during the first year of the demonstration, while 52 percent of the control agencies increased their cost per episode by five percent or more (see Figure IV.1). Thus, the impact results are not driven by a few agencies making major changes in their pattern of care but instead indicate a strong pattern of many agencies responding incrementally to financial incentives.

A surprising result, however, is that the cost per episode increased by more than five percent for 25 percent of the treatment agencies. While one would predict that some agencies will encounter unexpected changes that result in cost-per-episode increases, this proportion seemed high given that each agency is compared against its own cost history. The increased costs were generally driven by increases in *both* cost per visit and visits per episode. We examined the treatment agencies that had increased their cost per episode by more than five percent, and we found no discernable pattern in the types of agencies. Agencies that experienced increasing costs included both large and small agencies and urban and rural agencies. The only distinguishing characteristics among these agencies were that (1) four had experienced declines in total visit volume during the years prior to entering the demonstration, and (2) three others were members of the same organizational chain. This suggests that market and management characteristics were the most likely explanation for why agencies might be vulnerable to losing money during the first year of prospective payment.



TABLE IV.1  
IMPACTS ON COST PER EPISODE  
(Agency-Level Analysis)

	Treatment Agencies' Cost Per Episode (in Dollars)	Control Agencies' Cost Per Episode (in Dollars)	Difference Between Treatment and Control Agencies (in Dollars)
Demonstration Year Average	2,751	3,369	-618
Base Year Average	3,031	3,230	-199
Difference Between Demonstration Year and Base Year	-280	139	-419***

SOURCE: Medicare Cost Reports and Bill Records. Results based on DD models.

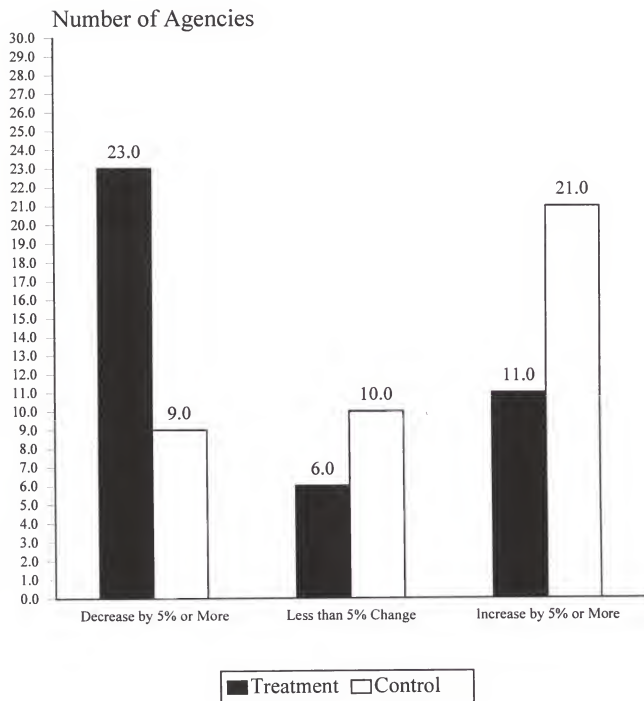
\*Significantly different from zero at the .10 level, two-tailed test.

\*\*Significantly different from zero at the .05 level, two-tailed test.

\*\*\*Significantly different from zero at the .01 level, two-tailed test.

FIGURE IV.1

DISTRIBUTION OF IMPACTS ON COST PER EPISODE



Note: Data from 80 agencies with episode and cost data from both years.

## B. HAVE CHANGES IN PATIENT CHARACTERISTICS AFFECTED COST PER EPISODE?

The results presented thus far indicate that the demonstration payment method greatly reduced the cost per episode of home health care. A key issue for policymakers, however, is whether these reductions in cost per episode are due to *changes* in patient characteristics.<sup>1</sup> If agencies are achieving these cost savings by serving a population that is healthier than the population they previously served, then the payment method holds less promise as a method to control Medicare costs. (In a nationwide system, all agencies cannot serve a healthier population unless access to care is denied for the less healthy.) Unfortunately, we cannot examine this issue directly because we have very limited data on individual patient characteristics in the base year.<sup>2</sup>

To consider the influence of patient characteristics, we estimate an OLS regression where the dependent variable is the cost per episode in the demonstration year, and the set of independent variables includes an extensive list of agency- and patient-level characteristics shown in Table II.2. The results from this regression, shown on the second row of Table IV.2, indicate that the demonstration payment method reduced the cost per episode by \$235, or about 8.7 percent. While this impact is large and statistically significant, it is far smaller than the impact of -\$419 (shown in Table IV.1) from a DD model that does not control for individual patient characteristics.

---

<sup>1</sup>The DD model (used to recover the impact reported in Table IV.1) controls for all agency and patient characteristics that remain fixed between the predemonstration and demonstration periods. The model may not effectively control for factors that change through time, however, such as patients' characteristics. For a discussion of this issue, see the methodology section of Chapter II.

<sup>2</sup>We have only the patient characteristics data that come from the Medicare files. These data do not include information on functioning and care needs, available for the demonstration year. See the data section of Chapter II for more details.

TABLE IV.2  
COMPARISON OF IMPACTS ON COST PER EPISODE  
UNDER ALTERNATIVE SPECIFICATIONS

	Predicted Mean in Absence of the Demonstration <sup>a</sup> (in Dollars)	Impact of Episode Rate Setting (in Dollars)	P-value <sup>b</sup>
D.D. Model	3,170	-419	(.01)
<b>Episode Level Analysis</b>			
Controlling for Agency and Patient Characteristics	3,170	-235	(.05)
Controlling for Agency Characteristics	3,170	-275	(.01)

SOURCE: Medicare Cost Reports and Bill Records.

<sup>a</sup>Calculated as the mean for treatment agencies in the base year plus the change for control agencies from the predemonstration to demonstration periods.

<sup>b</sup>The p-values for the episode-level analysis are based on standard errors that have been adjusted to account for the effects of weighting and clustering.

The much smaller estimate from the OLS model for the demonstration year suggests that patient characteristics have a major influence on the extent to which prospective payment reduces cost per episode; however, upon further examination, this inference proves to be false. When the OLS regression is reestimated *without* patient characteristics, the estimated impact of prospective payment on cost per episode is largely unchanged—a difference of just \$40 (Table IV.2). Thus, patient characteristics appear to have very little influence on the extent to which prospective payment reduces per episode costs, and they are not responsible for the large discrepancy in the DD and OLS estimates.

If the discrepancy is not the result of patient characteristics, what is its source, and which estimate is to be believed? The answer, in short, is that the larger impact of -\$419 (-13 percent) is more supportable because the DD model controls for far more factors that may bias the estimated demonstration impact. When we repeat the OLS regressions using episodes from the predemonstration period (not shown), we find that being a treatment agency has a positive and significant impact on the cost per episode even after controlling for observed agency characteristics. In fact, the impact estimate is \$169—almost exactly the discrepancy found between the DD estimate cost of -\$419 and the OLS estimate from the demonstration year (controlling for agency characteristics) of -\$275. Since the method of payment is the same for all agencies in the predemonstration period, this result indicates that there must be unobserved differences between treatment and control agencies that (on average) lead to higher per-episode costs for treatments. The DD results reported Table IV.1 control for these unobserved factors (assuming they are fixed) but the demonstration period regression does not. Thus, we conclude that the impact estimate from the OLS regression has been biased downward by these unobserved treatment-control differences.

Further, the findings from the DD models reflect the best estimates of the impact of prospective payment on the cost per episode during the initial months of the demonstration.

### **C. HOW DID AGENCIES ACHIEVE THE REDUCTION IN COST PER EPISODE?**

As discussed previously, prospectively paid agencies can reduce their per-episode costs through two methods. First, they may reduce their cost per visit for services rendered during the episode of care. Second, they may change the volume or mix of services provided per patient episode; this includes reducing the number of total visits provided and/or a changing the visit mix that favors less expensive types of care. Clearly, these methods are not mutually exclusive or independent. For example, large reductions in visits may lead to higher cost per visit, while efforts to cut cost per visit may force changes in the mix of services provided.

The actual method through which treatment agencies reduced per-episode costs has important policy implications. One possible, but unlikely, explanation is that treatment agencies achieved lower costs per episode entirely by reducing costs per visit, with no loss in the quantity or quality of care. The inference from such a finding is that the prospective payment led to large gains in service efficiency with no loss in care quality. A second possibility is that treatment agencies reduced both the cost per visit and the provision of visits in response to the demonstration. In this case, the policy implications depend largely on how care quality is affected.<sup>3</sup> If quality is unchanged, it suggests that treatment agencies have cut costs without adversely affecting beneficiaries, making prospective payment an attractive option. Similarly, if treatment agencies achieved cuts in per-episode costs through higher costs per visits but reduced visits, the desirability of prospective payment depends on the effects on quality.

---

<sup>3</sup>Chen et al. (1998) finds no impacts of the demonstration on quality during the first year of the demonstration.

This section discusses the extent to which the impact of the demonstration on cost per episode was driven by (1) changes in the cost per visit for each type of service, and (2) changes in the volume and mix of services provided. As discussed below, we find that the entire reduction in cost per episode was the result of the sharp decline in the volume of services provided. In fact, for visits by both home health aides and skilled nurses, costs per visit actually *increased* significantly as a result of prospective payment. These increases were largely independent of changes in agencies' total volume between the base year and the first year of the demonstration, suggesting that the growth in cost per visit was due to factors other than reduced economies of scale.

### **1. Effects on Cost Per Visit**

Base year averages for the cost of each type of visit, shown in Table IV.3, indicate that the cost per visit was nearly equal for treatment and control agencies prior to the demonstration. The cost per skilled nursing visit was in the mid-\$80s on average for both treatments and controls, while the cost for home health aide visits averaged about \$39. The cost per visit for therapy services also differed by only a small amount, with the largest difference (about \$8) for speech. The cost of a medical social worker visit averaged nearly \$155 for both treatments and controls.

Somewhat surprisingly, prospective payment led to large increases in cost per visit for at least some services during the first year of the demonstration (Table IV.3). Treatment agencies realized increases of about \$14 per skilled nursing visit (17 percent) and about \$3.40 per home health aide visit (9 percent) between the base year and the first demonstration year, compared to an increase of only \$4 per skilled nursing visit and a decline of about \$2 per home health aide visit for control agencies. These statistically significant treatment/control differences--about \$11 for skilled nursing and \$5.35 for home health aide visits--suggest that prospective payment increased cost per visit substantially in the first year of the demonstration. As a percentage of the expected cost for

TABLE IV.3  
IMPACTS OF THE DEMONSTRATION ON AVERAGE COST PER VISIT

	Treatment Agencies	Control Agencies	Difference (p-value)
Skilled Nursing	(n=40)	(n=40)	
Demonstration year average	\$97.57	\$89.82	\$7.74
Base year average	\$83.55	\$86.81	-\$3.26
Difference between demonstration and base year	\$14.02	\$3.01	\$11.01*** (.00)
Home Health Aide	(n=40)	(n=40)	
Demonstration year average	\$41.90	\$37.37	\$4.52
Base year average	\$38.52	\$39.34	-\$0.82
Difference between demonstration and base year	\$3.38	-\$1.97	\$5.35** (.01)
Physical Therapy	(n=39)	(n=38)	
Demonstration year average	\$107.44	\$99.68	\$7.76
Base year average	\$106.08	\$104.50	\$1.58
Difference between demonstration and base year	1.36	-\$4.82	\$6.18 (.72)
Speech Therapy	(n=34)	(n=36)	
Demonstration year average	\$106.05	\$107.73	-\$1.68
Base year average	\$93.92	\$102.33	-\$8.40
Difference between demonstration and base year	\$12.13	\$5.40	\$6.73 (.42)
Occupational Therapy	(n=34)	(n=35)	
Demonstration year average	\$116.41	\$117.80	-\$1.40
Base year average	\$93.60	\$95.70	-\$2.11
Difference between demonstration and base year	\$22.81	\$22.10	\$0.71 (.96)
Medical Social Worker	(n=37)	(n=39)	
Demonstration year average	\$160.87	\$157.84	\$3.03
Base year average	\$153.20	\$154.18	-\$0.97
Difference between demonstration and base year	\$7.67	\$3.66	\$4.00 (.86)
Administrative	(n=40)	(n=38)	
Demonstration year average	\$30.65	\$25.93	\$4.72
Base year average	\$26.85	\$25.13	\$1.72
Difference between demonstration and base year	\$3.80	\$0.80	\$3.00 (.49)

SOURCE: Medicare Cost Reports, based on DD models.

\*Significantly different from zero at the .10 level, two-tailed test.

\*\*Significantly different from zero at the .05 level, two-tailed test.

\*\*\*Significantly different from zero at the .01 level, two-tailed test.



treatment agencies in absence of the demonstration, these differences reflect impacts of about 13 percent and 15 percent, respectively, for these services.

There is some evidence that per-visit costs also increased for therapy and medical social worker visits, though none of the estimated impacts for these services were statistically significant. Within therapy services, the estimated impact on per-visit costs for physical therapy and speech therapy were about six percent and seven percent, respectively, while the impact on occupational therapy was negligible. For medical social workers, the estimated impact was about three percent. None of the p-values on these estimates were below 0.5.

#### **a. Source of Impacts on Cost Per Visit**

As discussed in Chapter II, the impact estimates shown in Table IV.3 may be the result of changes in case-mix or service volume not accounted for through a simple DD model. A finding that the impact estimates change when controlling for patient characteristics or service volume would suggest that the demonstration affects cost per visit indirectly through either the types of patients that receive care or the volume of care provided.<sup>4</sup> For instance, if we find that the rise in cost per visit resulting from the demonstration is weaker when controlling for service volume, it is likely that treatment agencies have lost economies of scale as a result of cutting visits.

When controlling for changes in volume and patient characteristics, prospective payment is still found to significantly increase the cost per skilled nursing visit and the cost per home health aide visit (Table IV.4). In fact, both of these impacts remain constant at about \$11 and \$5.30, respectively. This is perhaps surprising since it suggests that none of the growth in cost per visit

---

<sup>4</sup>Agency-level data on patient characteristics is limited to demographic information such as gender, race, and age. See Chapter II for more details.

TABLE IV.4

IMPACTS OF THE DEMONSTRATION ON COST PER VISIT,  
CONTROLLING FOR VOLUME AND PATIENT CHARACTERISTICS

	Predicted Mean in the Absence of the Demonstration <sup>a</sup>	Impact (Base Model)	Impact (Controlling for Volume and Patient Characteristics)
Skilled Nursing (p-value)	\$86.56	\$11.01** (.00)	\$10.78*** (.00)
Home Health Aide (p-value)	\$36.55	\$5.35** (.01)	\$5.38** (.01)
Physical Therapy (p-value)	\$101.26	\$6.18 (.72)	\$16.20 (.33)
Speech Therapy (p-value)	\$99.32	\$6.73 (.42)	\$3.38 (.68)
Occupational Therapy (p-value)	\$115.70	\$0.71 (.96)	\$6.38 (.63)
Medical Social Worker (p-value)	\$156.86	\$4.00 (.86)	\$1.89 (.94)
Administrative (p-value)	\$27.65	\$3.00 (.49)	\$2.57 (.50)

SOURCE: Medicare Cost Reports.

<sup>a</sup>Calculated as the mean for treatment agencies in the base year plus the change for control agencies from the predemonstration to demonstration periods.

\*Significantly different from zero at the .10 level, two-tailed test.

\*\*Significantly different from zero at the .05 level, two-tailed test.

\*\*\*Significantly different from zero at the .01 level, two-tailed test.

resulting from the demonstration is attributable to lost economies of scale or changes in patient characteristics.<sup>5</sup>

If the relative growth in cost per visit for treatment agencies is not the result of changes in patient characteristics or overall service volume, what is its source? A direct implication of our result that the demonstration raised cost per visit but reduced cost per episode is that agencies must have made large cuts in at least some services.<sup>6</sup> Thus, one likely explanation for the growth in per visit costs is that it is a byproduct of efforts by treatment agencies to provide more efficient delivery of *per-episode* services. If, for example, treatment agencies substituted telephone contacts for some visits or lengthened their average time spent per visit in an effort to reduce the number of visits, the unit costs for visits will increase. In order to investigate this possibility, we reestimated the impacts of the demonstration on cost per visit, controlling for the change in average visits provided per episode (both in total and by the specific service). The results from these models, shown in Table IV.5 are dramatic. The estimated impact of the demonstration on the cost of a skilled nursing visit falls by nearly half (from \$11 to \$5.80); likewise, the impacts on costs per visit for home health aides and physical therapists also decline substantially and are insignificant.

While reductions in visits per episode explain much of the demonstration impact on cost per visit, they do not explain it all. In particular, the impact on the cost per skilled nursing visit remains large and significant even after controlling for cuts in per-episode services. There are two general

---

<sup>5</sup>The limited patient characteristics available make it possible that the change in cost per visit is still due in part to changes in case mix. However, this appears unlikely, since the episode-level analysis on cost per episode (which used more detailed patient-level data from the demonstration year) also finds that patient characteristics matter little.

<sup>6</sup>As discussed in the next subsection, we find strong evidence to support this inference; specifically, treatment agencies made large cuts in visits per episode for nearly all services in response to demonstration payment method.

explanations for such “residual” effects. First, the demonstration may have introduced short-term transition costs as treatment agencies learned to operate under the new payment system. If, for example, the payment method requires additional training or greater initial oversight of staff, we would expect to see the per-visit costs of treatment agencies rise in relation to those of control agencies.<sup>7</sup> Second, these higher costs may be the result of longer-term increases created by the new payment system. For instance, as fewer visits are reduced per episode, the proportion of initial assessment visits (which are more costly) to total visits will increase, thus increasing overall average cost per visit.

Evidence from the site visits suggests that both factors contributed to the increase in cost per visit, but changes in long-term costs may be the more important factor. Both treatment and control agencies indicated that they incurred additional short-term costs as a result of the demonstration, and they generally cited the training cost for the Quality Assurance Instruments—which applied to both treatment and control agencies—as the biggest component of that cost. One cost cited, that of developing software to “split-bill” at 120 days, applied to the treatment agencies and not the control agencies.<sup>8</sup> However, we should caution that most agencies did not track these costs, and thus the evidence is just suggestive.

On the other hand, treatment agencies undertook a number of initiatives that would both increase the cost per visit and probably last. This includes closer supervision of service utilization (50 percent of agencies), development and training of staff to employ care maps (39 percent of agencies), increased use of telephone contacts (33 percent of agencies), and increased use of specialists (17 percent of agencies).

---

<sup>7</sup>This issue will be examined in detail in the final report as more years of data become available.

<sup>8</sup>During the demonstration, treatment agencies must split their bill at the end of 120 days to differentiate the visits covered by the episode payment and those covered by the outlier payment method.

TABLE IV.5

IMPACTS OF THE DEMONSTRATION ON AVERAGE COST PER VISIT,  
CONTROLLING FOR AVERAGE VISITS PER EPISODE

	Predicted Mean in the Absence of the Demonstration <sup>a</sup>	Impact (Base Model)	Impact (Controlling for Average Visits Per Episode)
Skilled Nursing (p-value)	\$86.56	\$11.01** (0.00)	\$5.76* (.10)
Home Health Aide (p-value)	\$36.55	\$5.35** (.01)	\$2.84 (.20)
Physical Therapy (p-value)	\$101.26	\$6.18 (.72)	\$4.28 (.81)
Speech Therapy (p-value)	\$99.32	\$6.73 (.42)	\$7.52 (.40)
Occupational Therapy (p-value)	\$115.70	\$0.71 (.96)	-\$10.80 (.44)
Medical Social Worker (p-value)	\$156.86	\$4.00 (.86)	-\$12.32 (.61)
Administrative (p-value)	\$27.65	\$3.00 (.49)	-\$2.00 (.66)

SOURCE: Medicare Cost Reports.

<sup>a</sup>Calculated as the mean for treatment agencies in the base year plus the change for control agencies from the predemonstration to demonstration periods.

\*Significantly different from zero at the .10 level, two-tailed test.

\*\*Significantly different from zero at the .05 level, two-tailed test.

\*\*\*Significantly different from zero at the .01 level, two-tailed test.

As data become available for later demonstration years and information is collected from site visits, it should be possible to identify the extent to which each of these factors is responsible for the remaining rise in cost per visit during the first year of demonstration. This is an important question, since the two explanations have potentially very different implications for the long-run impacts a prospective payment system will have on cost. In particular, if upward pressure on costs is short term, the positive impacts on cost per visit found in this initial period of the demonstration may dissipate over time. If this is the case, our current findings on cost per episode may very well reflect a lower-bound estimate of the long-run savings generated by prospective payment. Even if the higher costs persist, however, our estimates suggest that prospective payment substantially reduced agencies' cost per episode.

#### **b. Robustness**

A final issue concerns the effect of outliers on these findings. Because cost-per-visit figures are prone to substantial fluctuation between individual years, particularly in small agencies, it is important to consider the extent to which the results presented in this section have been driven by extreme changes in a handful of outlier agencies.<sup>9</sup> Observation of the sample data suggests that while such changes are evident, they are limited to at most three or four agencies for a given service. In order to consider what effect these outlier agencies have on our findings, we reestimated the demonstration impacts after dropping the two treatment and two control agencies on each tail of the cost-per-visit distribution for each type of visit. The results, given by Table IV.6, show somewhat smaller impacts on cost per visit for skilled nurses and home health aides when outliers are excluded, but these impacts remained significant and quite large. We therefore conclude that our findings on

---

<sup>9</sup>We also investigated the sensitivity of our results to possible non-billing by agencies and found no evidence that this has affected our results. See Chapter II for further details.

TABLE IV.6

## IMPACTS OF THE DEMONSTRATION ON AVERAGE COST PER VISIT, EXCLUDING OUTLIERS

	Predicted Mean in the Absence of the Demonstration <sup>a</sup>	Impact (Base Model)	Impact (Excluding Outliers)
Skilled Nursing (p-value)	\$86.56	\$11.01** (.00)	\$8.93*** (.00)
Home Health Aide (p-value)	\$36.55	\$5.35** (.01)	\$3.39*** (.01)
Physical Therapy (p-value)	\$101.26	\$6.18 (.72)	\$2.26 (.37)
Speech Therapy (p-value)	\$99.32	6.73 (.42)	\$6.33* (.10)
Occupational Therapy (p-value)	\$115.70	\$0.71 (.96)	\$4.92 (.27)
Medical Social Worker (p-value)	\$156.86	\$4.00 (.86)	-\$5.44 (.54)
Administrative (p-value)	\$27.65	\$3.00 (.49)	\$4.71*** (.00)

SOURCE: Medicare Cost Reports.

<sup>a</sup>Calculated as the mean for treatment agencies in the base year plus the change for control agencies from the predemonstration to demonstration periods.

\*Significantly different from zero at the .10 level, two-tailed test.

\*\*Significantly different from zero at the .05 level, two-tailed test.

\*\*\*Significantly different from zero at the .01 level, two-tailed test.

per-visits costs for skilled nurses and home health aides for these services are not simply a product of outlier effects. For the remaining services, the point estimates often changed by larger amounts when outliers were excluded from the sample, and the statistically insignificant impacts on speech therapy and administrative costs became significant. Since the significance of these impacts is driven largely by the reduced variance of the "trimmed" sample, however, we cannot conclude that they are evidence of demonstration impacts.

## **2. Effects on the Number of Visits Per Episode<sup>10</sup>**

Given the increases found in costs per visit, the decline in cost per episode resulting from prospective payment must be due to a reduction in the number of visits provided for at least some services. Clearly, the demonstration payment method gives agencies the incentive to reduce service provision. If prospectively paid agencies can cut the number of visits during the first 120 days of care without offsetting increases in per-visit costs, they will make a profit under the payment method. The control agencies have no such financial incentive.

As expected, we found large and significant treatment-control differences in the number of visits provided during the first 120 days of care (Table IV.7). Overall, prospective rate-setting led treatment agencies to provide an average of 8.5 fewer visits than control agencies. Relative to the mean for treatment agencies in absence of the demonstration (43.5 visits), this reflects an 19.5 percent decline in total visits. Much of this overall decline was the result of fewer visits by skilled nurses and home health aides. Compared to the number of visits provided by agencies in the control group, prospectively paid agencies provided an average of 4.3 fewer skilled

---

<sup>10</sup>Chen et al. (1997) provide complete details on the results presented in this subsection. The impacts presented here differ slightly, because of sample differences; however, they are qualitatively equivalent.



TABLE IV.7

## IMPACTS OF THE DEMONSTRATION ON THE NUMBER OF VISITS PER EPISODE

	Treatment Agencies	Control Agencies	Difference (p-value)
Total			
Demonstration year average	35.0	46.3	-11.3
Base year average	43.1	45.9	-2.8
Difference between demonstration and base year	-8.1	0.4	-8.5*** (.00)
Skilled Nursing			
Demonstration year average	17.0	22.5	-5.5
Base year average	21.1	22.3	-1.2
Difference between demonstration and base year	-4.1	0.2	-4.3*** (.00)
Home Health Aide			
Demonstration year average	11.9	16.0	-4.1
Base year average	15.1	15.9	-0.8
Difference between demonstration and base year	-3.2	0.1	-3.3*** (.00)
Physical Therapy			
Demonstration year average	4.7	5.7	-1.0
Base year average	5.1	5.6	-0.5
Difference between demonstration and base year	-0.4	0.1	-0.5 (.31)
Speech Therapy			
Difference between demonstration and base year	0.3	0.3	0.0
Base year average	0.4	0.3	0.1
Difference between base year and demonstration year	-0.1	0.0	-0.1 (.73)
Occupational Therapy			
Demonstration year average	0.6	1.0	-0.4
Base year average	0.8	1.0	-0.2
Difference between demonstration and base year	-0.2	0.0	-0.2 (.15)
Medical Social Worker			
Demonstration year average	0.5	0.8	-0.3
Base year average	0.6	0.8	-0.2
Difference between demonstration and base year	-0.1	0.0	-0.1 (.37)

SOURCE: Medicare Bill Records.

\*Significantly different from zero at the .10 level, two-tailed test.

\*\*Significantly different from zero at the .05 level, two-tailed test.

\*\*\*Significantly different from zero at the .01 level, two-tailed test.

nursing visits and 3.3 fewer home health aide visits during the at-risk period of each episode, statistically significant declines of about 20 percent and 22 percent respectively. Prospective rate-setting also led to estimated declines in the other services as well, though these were not statistically significant.

The estimated impacts when using episode-level data from the demonstration period (Table IV.8) were somewhat smaller than those shown in Table IV.7 using agency-level data.<sup>11</sup> Like the discrepancy found in cost per episode, however, these smaller impacts appear to be the result of estimation bias in the episode-level estimates. Specifically, when examining episode-level data from the *predemonstration period*, we find treatment agencies delivered on average about two more visits per episode. Since the DD approach controls for this preexisting treatment-control difference, the estimates in Table IV.7 are unaffected, but the estimates using the demonstration data are biased downward.

#### **a. Robustness**

Given the results on cost per visit, the finding that prospective payment greatly reduced visits is entirely consistent with the moderate decline found in cost per episode. Nevertheless, it is worth investigating whether these effects are sensitive to the specification of the model or the influence of outliers. Results from various sensitivity tests suggest that all the impacts on the provision of per-episode services were highly robust.<sup>12</sup> Estimates from the agency-level analysis that controlled for outliers showed little change in estimated impacts. In addition, estimates from the episode-level data that (1) weighted agencies by their proportion of demonstration episodes, (2) accounted for

---

<sup>11</sup>These “levels” models are described in the methods section of Chapter II. Their advantage is that they control for more detailed case-mix information and exploit the subsample of episodes within agencies.

<sup>12</sup>See Cheh et al. (1997) for details on these robustness tests.

TABLE IV.8  
IMPACTS OF THE DEMONSTRATION ON THE NUMBER OF VISITS PER EPISODE  
(EPISODE-LEVEL ANALYSIS)

	Predicted Mean in the Absence of the Demonstration <sup>a</sup>	Impact (Base Model)	Impact (Episode Data) <sup>b</sup>
Total (p-value)	43.5	-8.5*** (.00)	-6.5*** (.00)
Skilled Nursing (p-value)	21.3	-4.3** (.00)	-3.3*** (.00)
Home Health Aide (p-value)	15.2	-3.3*** (.00)	-2.1** (.04)
Physical Therapy (p-value)	5.2	-0.5 (.31)	-0.7 (.23)
Speech Therapy (p-value)	0.4	-0.1 (.73)	-0.1 (.19)
Occupational Therapy (p-value)	0.8	-0.2 (.15)	-0.2 (.31)
Medical Social Worker (p-value)	0.6	-0.1 (.37)	-0.3** (.02)

SOURCE: Medicare Cost Reports and Bill Records.

<sup>a</sup>Calculated as the mean for treatment agencies in the base year plus the change for control agencies from the predemonstration to demonstration periods.

<sup>b</sup>The p-values for the episode data are based on standard errors adjusted to account for the effects of clustering and weighting.

\*Significantly different from zero at the .10 level, two-tailed test.

\*\*Significantly different from zero at the .05 level, two-tailed test.

\*\*\*Significantly different from zero at the .01 level, two-tailed test.

censoring of the dependent variable, or (3) controlled for the effects of outliers were all consistent with the findings discussed above.

### **3. What Was the Relative Impact of These Two Effects?**

The evidence presented thus far indicates that the demonstration impact on cost per episode was the result of two countervailing forces: a decrease in the volume of services per episode and an increase in the cost per visit. Understanding the relative magnitude of these impacts can be insightful for policymakers as they design the new payment system. Previous results provide only limited and imprecise information on this relationship, because the overall impact on cost per episode is a (non-linear) combination of individual demonstration effects on cost per visit and visit per episode for each of the six services.

In order to decompose the change in cost per episode into the changes in cost per visit and visit per episode, we calculate the "share" of each individual effect as follows. To calculate the cost per visit share in each agency, we multiply the change in the cost per visit (for each type of service) by the average visits per episode in the base year. Similarly, to calculate the visit-per-episode share in each agency, we multiply the change in visits per episode (for each type of service) by the base-year cost per visit. Summing across each of the visit types, we then obtain for each agency the share of the cost per episode impact attributable to the two components.<sup>13</sup> By averaging the shares separately among treatment agencies and control agencies, we obtain the final estimates of the relative magnitude of the two impacts on cost per episode.

---

<sup>13</sup>We allocate the remaining "interaction" term (reflecting the change in cost per visit multiplied by the change in visits) proportionally across the two shares after summing across visits. So, for example, if the total impact on cost per episode is -\$150, and the shares are \$100 and -\$200 (a 1:2 ratio), the respective "final" shares are given as \$150 and -\$300.

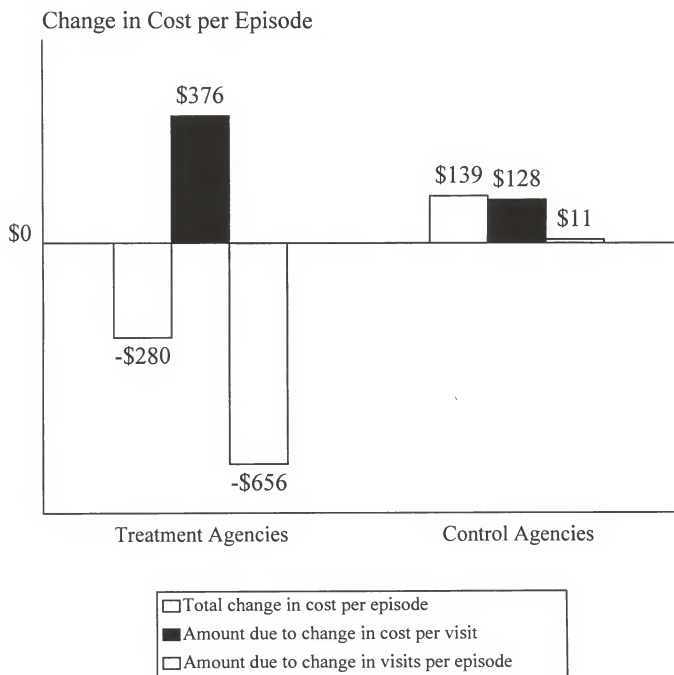
Results from this decomposition (shown in Figure IV.2) indicate that among treatment agencies, the large decline in the cost per episode (-\$280, or about 9 percent) was the result of two large but offsetting effects: rising cost per visit that drove the cost per episode up \$376, and decreases in visits per episode that reduced the cost per episode by \$656. Among control agencies, the effects on cost per episode were far less dramatic. A relatively small increase in cost per episode (\$139, or about 4 percent) was due to rising costs among both factors, with most of the increase (\$128) coming from higher cost per visit.

Taken together, these findings suggest that the demonstration payment method had a profound effect on agency behavior (Figure IV.2). Control agencies pursued "business as usual" during the demonstration, and as a result they experienced only marginal increases in their costs. Treatment agencies, however, changed the way that they provided care, and as a result their cost components changed dramatically. Overall, treatment agencies did achieve large cuts in cost per episode; however, more than half the overall savings generated from reduced visits per episode were offset by an escalation in cost per visit.

Taking the difference in the change between treatment and control agencies (-\$280 to \$139), we arrive at the estimated impact of the demonstration on the cost per episode, -\$419 (shown in Table IV.1). Calculating the equivalent treatment-control difference for the change in cost per visit and visits per episode, we find that this demonstration impact was the result of (1) a rise of \$248 in per-episode costs due to increased cost per visit; and (2) a decline of \$667 in episode costs due to reduced visits per episode. Relative to the "base" cost per episode of \$3,170, these figures reflect

FIGURE IV.2

# DECOMPOSITION OF IMPACTS ON COST PER EPISODE



Note: The amount due to the interaction of cost per visit and visits per episode is allocated proportionately between the two effects.

an 8 percent reduction in episode costs due to increases in cost per visit and a 21 percent decline in episode costs due to decreases in visits per episode, or a net effect of -13 percent.<sup>14</sup>

#### **D. DID DIFFERENT TYPES OF AGENCIES RESPOND TO THE DEMONSTRATION DIFFERENTLY?**

Agencies are likely to respond to the demonstration incentives differently, depending on their characteristics. We focus on four characteristics that we believe are the most likely to affect agency behavior: (1) whether an agency is for-profit or nonprofit, (2) whether it had a predemonstration practice pattern index value above (high-use) or below (low-use) the median value, (3) whether it provided more or fewer than 30,000 visits in the base year, and (4) whether it is freestanding or hospital-based. For each of these subgroups, we examine the differences in the effects of payment on each of the following outcomes: cost per episode, cost per visit for skilled nursing and home health aide visits, and the number of visits per episode. All of the results presented in this section are based on DD models, estimated individually for the various subgroups. Impacts derived from an episode-level analysis that examined subgroups simultaneously showed similar results.

Proprietary agencies have the clearest incentive to cut their cost per episode under the demonstration. Nonprofit agencies have more diverse objectives; while they might like to earn a surplus to support charity work, they might reduce costs per episode by a smaller fraction than for-profit agencies, because such reductions could conflict with their mission. Prospective payment could result in no change in cost per episode for nonprofit agencies, especially in light of the loss protection available under the demonstration. As a result, the responses to the demonstration from for-profit and nonprofit agencies may differ substantially.

---

<sup>14</sup>The base is calculated as the mean for treatment agencies in absence of the demonstration, or \$3,031 (the base-year cost per episode for treatment agencies) + \$139 (the change in the control group mean between the predemonstration and demonstration periods).

Agencies with preexisting high-use practice patterns may be able to respond more aggressively to the demonstration payment method. In particular, those agencies that have not faced pressure from managed care organizations (and other competitive market forces) have had little incentive to contain costs under cost-based reimbursement. Under prospective payment, these agencies may therefore be more capable of making large-scale reductions in the cost of care that they provide per episode.

We also expect that smaller agencies (those under 30,000 visits per year) may respond differently to the demonstration incentives, although the direction of that response is uncertain. In general, smaller agencies are likely to find it more difficult to cut visits without realizing substantial increases in cost per visit, because they would be more prone to reduce economies of scale. Thus, we might expect that smaller agencies would realize a less substantial decline in cost per episode. However, because smaller agencies may find it easier to communicate with their staff and implement changes in care patterns more quickly, they may be able to make larger reductions in visits than larger agencies, at least in the short run.

Hospital-based agencies have different financial incentives, so they may respond to prospective payment differently. These agencies have the incentive to maximize profits for the entire hospital system, so their practices may differ from those of freestanding agencies. For example, a hospital-based system may substitute home health care for hospital days, with little regard for how this affects its home health agencies' costs.

The analysis of subgroups (Tables IV.9 through IV.12) shows one clear difference in agency response to the demonstration: Large home health agencies were able to lower their cost per episode significantly more than small agencies (Table IV.11). Among large agencies, the average cost per episode decreased by \$620, a significant decline of 20 percent relative to the control group mean for



TABLE IV.9

IMPACT OF PER-EPISEDE PAYMENT ON THE COST PER EPISEDE,  
BY WHETHER THE AGENCY IS FOR-PROFIT OR NONPROFIT

	Control Group Mean	Impact <sup>a</sup> (p-value) <sup>b</sup>
Cost Per Episode		
For-profit	\$3,804	-\$315
Nonprofit	\$2,595	-\$510**
p-value for impact differences between subgroups		(.52)
Cost Per Skilled Nursing Visit		
For-profit	\$88.46	\$6.97*
Nonprofit	\$85.00	\$15.04**
p-value for impact differences between subgroups		(.23)
Cost Per Home Health Aide Visit		
For-profit	\$41.45	\$7.02*
Nonprofit	\$37.01	\$3.60*
p-value for impact differences between subgroups		(.42)
Total Number of Visits		
For-profit	52.6	-6.8*
Nonprofit	38.5	-10.1***
p-value for impact differences between subgroups		(.47)

SOURCE: Medicare Cost Reports and Bill Records. Sample size: 40 for-profit agencies, 40 nonprofit agencies.

<sup>a</sup>These estimates, obtained from agency-level DD models, represent treatment-control differences for each of the outcomes listed in the first column.

<sup>b</sup>The p-value for each outcome corresponds to a test of whether the impacts for the two subgroups are statistically different from one another.

\*Significantly different from zero at the .10 level, two-tailed test.

\*\*Significantly different from zero at the .05 level, two-tailed test.

\*\*\*Significantly different from zero at the .01 level, two-tailed test.

TABLE IV.10

IMPACT OF PER-EPIISODE PAYMENT ON THE COST PER EPIISODE,  
BY WHETHER THE AGENCY HAD A HIGH-USE OR  
LOW-USE PRIOR PRACTICE PATTERN

	Control Group Mean	Impact <sup>a</sup> (p-value) <sup>b</sup>
Cost Per Episode		
High-use practice pattern	\$3,443	-\$379*
Low-use practice pattern	\$2,353	-\$457**
p-value for impact differences between subgroups		(.80)
Cost Per Skilled Nursing Visit		
High-use practice pattern	\$85.53	\$3.47
Low-use practice pattern	\$88.95	\$16.93***
p-value for impact differences between subgroups		(.05)
Cost Per Home Health Aide Visit		
High-use practice pattern	\$37.36	\$1.94
Low-use practice pattern	\$42.65	\$8.74**
p-value for impact differences between subgroups		(.12)
Total Number of Visits		
High-use practice pattern	52.8	-7.9**
Low-use practice pattern	32.7	-9.1***
p-value for impact differences between subgroups		(.60)

SOURCE: Medicare Cost Reports and Bill Records. Sample size: 40 high-use agencies, 40 low-use practice pattern agencies.

NOTE: An agency is defined as having a high-use practice pattern if its (case-mix adjusted) number of visits per episode in the base (predemonstration) period was above the median for all agencies in the sample. Otherwise, it is defined as having a low-use practice pattern.

<sup>a</sup>These estimates, obtained from (agency-level) DD models, represent treatment-control differences for each of the outcomes listed in the first column.

<sup>b</sup>The p-value for each outcome corresponds to a test of whether the impacts for the two subgroups are statistically different from one another.

\*Significantly different from zero at the .10 level, two-tailed test.

\*\*Significantly different from zero at the .05 level, two-tailed test.

\*\*\*Significantly different from zero at the .01 level, two-tailed test.

TABLE IV.11

IMPACT OF PER-EPISODE PAYMENT ON THE COST PER EPISODE,  
BY WHETHER THE AGENCY IS SMALL OR LARGE SIZE

	Control Group Mean	Impact <sup>a</sup> (p-value) <sup>b</sup>
Cost Per Episode		
Small size	\$3,557	-\$39
Large size	\$3,148	-\$620***
p-value for impact differences between subgroups		(.09)
Cost Per Skilled Nursing Visit		
Small size	\$81.80	\$18.42*
Large size	\$88.07	\$7.64**
p-value for impact differences between subgroups		(.16)
Cost Per Home Health Aide Visit		
Small size	\$40.57	\$9.82
Large size	\$39.04	\$4.37**
p-value for impact differences between subgroups		(.26)
Total Number of Visits		
Small size	56.2	-7.3
Large size	43.3	-9.9***
p-value for impact differences between subgroups		(.61)

SOURCE: Medicare Cost Reports and Bill Records. Sample size: 22 small agencies, 58 large agencies.

NOTE: Large (small) agencies are defined as those that provided more (fewer) than 30,000 visits in the predemonstration year.

<sup>a</sup>These estimates, obtained from (agency-level) DD models, represent treatment-control differences for each of the outcomes listed in the first column.

<sup>b</sup>The p-value for each outcome corresponds to a test of whether the impacts for the two subgroups are statistically different from one another.

\*Significantly different from zero at the .10 level, two-tailed test.

\*\*Significantly different from zero at the .05 level, two-tailed test.

\*\*\*Significantly different from zero at the .01 level, two-tailed test.

TABLE IV.12

IMPACT OF PER-EPISEDE PAYMENT ON THE COST PER EPISEDE,  
BY WHETHER THE AGENCY IS HOSPITAL-BASED  
OR FREESTANDING

	Control Group Mean	Impact <sup>a</sup> (p-value) <sup>b</sup>
Cost Per Episode		
Hospital-based	\$2,923	\$142
Freestanding	\$3,274	-\$456***
p-value for impact differences between subgroups		(.32)
Cost Per Skilled Nursing Visit		
Hospital-based	\$98.01	\$15.76
Freestanding	\$85.21	\$10.58***
p-value for impact differences between subgroups		(.69)
Cost Per Home Health Aide Visit		
Hospital-based	\$40.30	\$11.87
Freestanding	\$39.21	\$4.36**
p-value for impact differences between subgroups		(.36)
Total Number of Visits		
Hospital-based	37.6	-2.2
Freestanding	47.1	-8.7***
p-value for impact differences between subgroups		(.46)

SOURCE: Medicare Cost Reports and Bill Records. Sample size: 7 hospital-based agencies, 73 freestanding agencies.

<sup>a</sup>These estimates, obtained from (agency-level) DD models, represent treatment-control differences for each of the outcomes listed in the first column.

<sup>b</sup>The p-value for each outcome corresponds to a test of whether the impacts for the two subgroups are statistically different from one another.

\*Significantly different from zero at the .10 level, two-tailed test.

\*\*Significantly different from zero at the .05 level, two-tailed test.

\*\*\*Significantly different from zero at the .01 level, two-tailed test.

this subgroup. In contrast, the average cost per episode showed essentially no change among small agencies. This difference between small and large agencies is due to both a smaller increase in cost per visit among large agencies and a larger reduction in visits. While neither of the differences in cost per visit or visits per episode is statistically significant, taken together they lead to the overall result that large agencies cut per-episode costs significantly while small agencies did not.

Substantial differences are also evident between freestanding and hospital-based agencies (Table IV.12). These differences are statistically insignificant, however, largely because of the very small sample of hospital-based agencies available. For freestanding agencies, cost per episode declined significantly (by \$456), while for hospital-based agencies, the impact estimate is actually positive (though very imprecise). Consistent with these results, freestanding agencies realized higher cuts in volume and lower increases in cost per visit than hospital-based agencies; however, these differences were again not significant.

We find no significant differences in impacts on cost per episode by profit status or preexisting practice patterns (Tables IV.9 and IV.10). In fact, for both pairs of subgroups, the impacts are very similar to those found for agencies in general. In each subgroup, the demonstration led to significant reductions in cost per episode, the net result of a general increase in cost per visit and a large decrease in visits.

We do find significant evidence that agencies with low use practice patterns realized larger increases in the cost per skilled nursing visit than agencies with high use practice patterns. Since low-practice pattern agencies provided relatively few visits per episode in the base year, this result suggests that these agencies could cut visits only through costly measures such as lengthening individual visits or substituting telephone contacts. High-practice pattern agencies, on the other hand, may have had more "room" to cut visits outright, thus leading to little or no increase in cost per visit.

## V. SUMMARY AND CONCLUSIONS

This preliminary analysis of demonstration impacts on the cost of Medicare home health services provides HCFA with critical information for the design of the prospective payment system mandated for implementation in 2000. Our findings are robust, which suggests that they will provide a good guide for policymakers, despite their preliminary nature.

### A. KEY FINDINGS

We find strong evidence that prospective payment substantially reduced the cost per episode. The cost per episode for prospectively paid agencies was \$419 less than the cost per episode for cost-reimbursed agencies: a 13 percent reduction in the cost of care. This reduction does not appear to be the result of changes in the types of patients served, because controlling for patient characteristics does not affect the size of the impact.

The reduction in the cost per episode is the result of two countervailing effects: (1) a decrease in the number of services rendered, and (2) an increase in the cost per visit. Specifically, the \$419 decline in the cost per episode reflects a \$667 decline in the cost per episode due to decreases in the use of services, which is partially offset by an increase of \$248 in the cost per episode due to cost-per-visit increases. The reduction in service use involved large and statistically significant declines in the number of visits by skilled nurses and home health aides, as well as large but statistically insignificant declines in the number of visits by physical therapists, occupational therapists, and medical social workers. In generating these reduced visit levels, however, treatment agencies experienced significantly higher costs per visit for skilled nursing care and home health aide care. The cost per visit for other Medicare-reimbursed services also increased, but these results were not statistically significant.

The impact on the cost per episode across different types of agencies, including proprietary/nonprofit and hospital-based/freestanding, is fairly similar to the overall impact. The one exception is that small agencies, those that rendered more than 30,000 visits in the base year, did not decrease their cost per episode in the first demonstration year. The evidence suggests that the cost per visit increased more for smaller agencies, whereas the number of visits per episode was reduced to a lesser extent.

## **B. POLICY IMPLICATIONS**

Our findings suggest that prospective payment for home health care could lead to sizable savings in costs to HCFA, depending on how the payment rates are set. These savings from the reduction in cost per episode during the at-risk period might be mitigated by changes in the patterns of care in the outlier period.<sup>1</sup> Nonetheless, according to the evidence presented here, the potential exists both for HCFA to save and for agencies to prosper.

Policymakers must remember that these agencies achieved cost savings by trading off decreases in visits for increases in per-visit costs. The old adage applies: one must spend money to make money. Supervising utilization more carefully, buying and implementing care-mapping protocols, and making telephone calls in lieu of visits all cost money. If the prospective payment system is going to work, it must be flexible enough that agencies can pay for the changes needed. The Interim Payment System (currently in effect) is an example of a system that is unlikely to achieve maximum cost savings, because it restricts flexibility. By imposing stricter limits on agencies' cost per visit, IPS effectively penalizes agencies that could achieve large cost per episode reductions using

---

<sup>1</sup>The issue of outlier payments will be addressed in future reports when data are available.

strategies that coincidentally raise per-visit costs--the exact strategies adopted by treatment agencies under the demonstration.

Furthermore, the results presented here suggest that because of a combination of two problems, small agencies may have difficulty thriving under prospective payment. First, small agencies have fewer episodes, so average levels of use will be much more volatile from year to year. Although small agencies may be able to manage levels of use over the long run, in any given period, they will have greater difficulty. Second, small agencies face more pronounced economies of scale; that is, when small agencies decrease their overall volume of services, their costs per visit increase more than those of large agencies. Indeed, research has shown that economies of scale may exist only for small agencies; changes in volume do not affect the large agencies' cost per visit (Chu, Brown, and Phillips 1993).

Nevertheless, under prospective payment, some small agencies may be able to reduce their cost per episode and still remain successful. This, however, probably is not true for many small agencies because of the fixed costs associated with operating a home health agency. Some small agencies will find it in their best interest to merge with other agencies once prospective payment has been adopted. But, agencies that serve *sparsely populated* areas may have only limited opportunities to reduce the cost per episode because of their small potential client base. An efficiently run agency in a large, sparsely populated geographic area may not be able to reduce its costs to the level of larger agencies even if, on average, the agency has similar patterns of care. Thus, the risk is that small agencies may leave the business under prospective payment and limit Medicare beneficiaries' access to care.

HCFA may wish to consider a special arrangement for agencies located in areas in which they are the only Medicare home health provider and where there is an insufficient population within a specified geographic limit (for example, 50-mile radius) to require a minimal number of visits per



year. One approach would be to implement in such areas a modified version of the profit and loss sharing arrangement used in the demonstration. This provision would motivate selected agencies to provide care efficiently while limiting the yearly swings in profitability that can affect solvency. Note, however, that any special arrangement would be difficult to implement. Such an arrangement would, for instance, require statutory authority and would create payment inconsistencies objectionable to some agencies. Moreover, any arrangement would need to be carefully designed to avoid subsidizing inefficient providers.

### **C. LIMITATIONS OF THE ANALYSIS**

Although we are confident that the estimates reflect the real effects of prospective payment on home health agency behavior, three potential limitations should be kept in mind. First is, can the results be generalized to other agencies? As in any study in which the participants are volunteers, demonstration agencies may be those best able to respond to the incentives of the demonstration. To the extent that this is true, the impacts of prospective payment, if instituted nationally, are likely to be smaller than those estimated here. On the other hand, our subgroup analysis suggests that, if agencies participating in the demonstration were disproportionally small, reductions in costs per episode under a national program could be larger than those estimated here. We have no evidence that participants are unrepresentative of agencies nationally, but we will explore this issue in a future report.

Second, we are concerned that the results may not provide a reliable guide to how agencies would behave under a national program, because of the ways in which such a program would likely differ from the demonstration program. Under a national program of prospective rate-setting, agencies would not be protected from incurring financial losses, which could compel some to respond more aggressively to prospective payment. Furthermore, the per-episode rate paid to an

agency probably would be based not on its own prior cost per episode, but on a regional or national average, thus greatly increasing the potential for losses for agencies with high-use practice patterns or high costs per visit.<sup>2</sup> The financial pressure on agencies would be intensified if the rate is based on the episode cost of an “efficient” agency or if policymakers--in an attempt to share in the savings that per-episode payment will generate--sets the per-episode payment at some percentage (for example, 90 percent) of the average national per-episode cost. Additional financial pressure on agencies, however, suggests that prospective payment effects might be even greater than those observed here.

Finally, these results are preliminary, which limits the inferences that can be drawn on several dimensions:

- The analysis covers only the first eight months of the three-year demonstration period for participating agencies. Impacts may change as agencies become more adept at, and comfortable with, finding ways to reduce the cost per episode.
- These results provide no information on the consequences of the reduced level of services for home health recipients or their families (for example, quality of care, access, and caregiver burden may be affected) or communities (use of community services may increase). Preliminary evidence shows that health outcomes were not affected by the demonstration (Chen 1998).
- The estimated reductions in cost per episode do not necessarily translate into proportionate reductions in Medicare program costs, because this analysis does not take into account changes in the cost of care during the outlier period or retrospective changes in payment rates due to the case-mix adjuster. Program effects on these other factors must be investigated as well. Costs for other Medicare-covered services may also be affected, although preliminary evidence suggests there were no such effects in the first demonstration year (Schore 1997).

These issues will all be addressed in future reports, as the required data become available.

---

<sup>2</sup>The demonstration included the loss protection and agency-specific rate setting measures to ensure participation.

## REFERENCES

- Cheh, Valerie, Christopher Trenholm, Randall Brown, and Barbara Phillips. "Preliminary Report: The Impact of Prospective Payment on Medicare Home Health Use: Promising Results for a Future Program. Princeton, NJ: Mathematica Policy Research, Inc., November 1997.
- Chen, Arnold, and Helaine Noreck. Preliminary Report: The Impact of Prospective Payment on Medicare Home Health Quality of Care Report." Report submitted to Health Care Financing Administration. Princeton, NJ: Mathematica Policy Research, Inc., January 1998.
- Chu, Dexter, Randall Brown, and Barbara Phillips. "Economics of Scale and Volume Adjustment for Medicare-Certified, Free-Standing Home Health Agencies." Report submitted to the Health Care Financing Administration. Princeton, NJ: Mathematica Policy Research, Inc., January 1993.
- Health Care Financing Review. *Medicare and Medicaid Statistical Supplement*, 1996.
- Phillips, Barbara, and Rachel Thompson. "Transition Within a Turbulent System: An Analysis of the Initial Implementation of the Per-Episode Home Health Prospective Payment Demonstration." Princeton, NJ: Mathematica Policy Research, Inc., July 1997.
- Schore, Jennifer. "Regional Variation in the Use of Medicare Home Health Services." In *Persons with Disabilities: Issues in Health Care Financing and Service Delivery*, edited by Wiener et al. Washington, DC: The Brookings Institution, 1995.
- Schore, Jennifer. "The Impact of Prospective Payment on Medicare Service Use and Reimbursement During the First Demonstration Year." Princeton, NJ: Mathematica Policy Research, Inc., December 9, 1997
- Teplitsky, Sanford V., and Mary Ann Janson (eds.). *Home Health & Hospice Manual: Regulations and Guidelines*. Owings Mills, MD: National Health Publishing, 1985-1992.

**APPENDIX A**

**SUMMARY OF CASE-MIX ADJUSTMENT  
FOR PAYMENTS DURING THE  
DEMONSTRATION**

During the demonstration, agency payment rates are adjusted for severity using the Home Health Utilization Groups (HHUGs) Classification system. This system, which was specifically developed for the demonstration, classifies patients into 1 of 18 mutually exclusive cells on the basis of the following information:

- Whether the patient has an intervening hospital stay in an acute-care hospital during the 120 days following a home health admission
- Whether the patient was impaired in at least four of five activities of daily living (bathing, eating/tube feeding, dressing, toileting/elimination, and transferring) at home health admission
- Whether wound care was planned for the patient at home health admission
- Whether the patient was discharged from a hospital within the 14 days preceding home health admission
- Whether the patient had stage 3 or stage 4 decubitus ulcer at home health admission
- Whether the patient had cancer at home health admission that affected current treatment or personal-care needs
- Whether the patient has had a cerebrovascular accident that affected current treatment or personal-care needs
- Whether the patient had diabetes that affected current treatment or personal-care needs

As with payment rates, demonstration case-mix adjustment is based on the agency's base-period experience. Information on the characteristics listed above was collected on each patient admitted to each agency in the 90 days ("base quarter") before it began to operate under the demonstration. The same information was collected throughout the demonstration in the "remarks" section of the Uniform Bill, HCFA form 92 (UB-92).

Using the base quarter information, Abt calculated a base quarter index for each agency as follows. First, Abt calculated a category weight for each of the 18 case-mix groups by dividing the

average cost in each group by the agency's overall average cost.<sup>1</sup> Abt then multiplied each category weight by the percentage of episodes that fell into each case-mix category during the base quarter, and summed across all 18 categories. The result is a base quarter index for each agency, which is always equal to 1.

To obtain the annual case-mix-adjusted rates, Abt calculated a similar index for the demonstration years by multiplying the category weight from the base year times the percentage of episodes that fell into each case-mix category during the relevant year, and then summing across all 18 categories.

The demonstration year index for each agency is then divided by the agency's base quarter index and multiplied by the agency's base year episode rate. This results in the case-mix-adjusted episode rate.

---

<sup>1</sup>If an agency did not have any observations in a particular category during the base quarter, the average cost for that casemix group calculated across all demonstration agencies was used instead of an agency-specific cost.

CMS LIBRARY



3 8095 00005972 1